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PROCEEDINGS

OF

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

PITTSBURG, PA.

VOL. VIII.

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ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

This Society does not hold itself responsible for the opinions of its members.

TWELFTH ANNUAL MEETING.

PITTSBURG, JANUARY 19TH, 1892.

THE Society met at the rooms of the Academy of Science and Arts, Fifth Street below Penn Avenue, at 8 P.M., with President T. P. Roberts in the chair, 49 members and visitors present.

The Treasurer's report was read and accepted. It was as follows:

RECEIPTS.

Balance from last year,	$119.69	
Dues for year ending Jan., 1890,	20.00	
" " " 1891,	172.50	
" " " 1892,	1210.00	
" " " 1893,	30.00	
Second dividend, Penn Bank,	14.93	
Extra papers sold,	29.18	
		$1596.30

EXPENDITURES.

Printing,	$626.15	
Rent of Rooms,	170.00	
Salary of Secretary,	170.00	
Postage and Office Expenses,	146.13	
Periodicals, English and American,	100.05	
" German and French,	44.60	
Stenographer,	50.00	
Repairs and Refitting,	18.12	
Commission on Collections,	16.75	
Letter-Press,	5.94	
Books for Library,	2.00	
Balance in hands of Treasurer,	246.56	
		$1596.30

A. E. FROST,
Treasurer.

The Auditing Committee reported that they had examined the books of the Treasurer and found them correct.

The Secretary's report for the year 1891 was received. It was as follows:

On January 20, 1890, the number of names on the list of members was	370
During the year were added	37
Aggregating,	407

From these 407 we lost 37; 25 who were dropped from the roll at the last annual meeting, 3 by death, and 9 by resignation, leaving on our roll 370.

Ten regular meetings were held, at which the following papers were read:

January 20. "Address" by the retiring President, W. L. Scaife, and a paper on "Description of an Electric Coal-Mining Clamp," by Charles H. Scott.

March 17. "Co-operation on Machine Designs," by Phineas Barnes; and the "Fales Grate," by H. D. Steinmetz.

April 19. "Minority Report of the Committee on Roads," by A. Kirk.

May 19. "Slow Combustion Construction of Buildings," by H. B. Chess.

June 16. "Bridge Designs," by H. J. Lewis.

October 20. "Steam Economy in Rolling-Mill Engine Practice," by D. Ashworth.

November 17. "Hydraulic Cements," by A. E. Hunt.

December 15. "Bridge Details," by E. Swensson.

The ten meetings were attended by 385 members and visitors, making an average attendance of 38.5 at each meeting.

J. H. HARLOW,
Secretary.

The Committee on Library reported; the report was accepted and ordered filed. It was as follows:

REPORT OF LIBRARY COMMITTEE.

President Eng. Soc. W. Pa.

Sir: On behalf of the Committee on Library we have to report as follows:

The total number of books added to the library during the year 1891 was 68. 51 periodicals were received. Of this number 19 came in exchange for the Society's *Proceedings*. The remaining 32 were obtained by subscription.

In reporting upon progress for the past year, it would have been highly gratifying to the Committee had we been able to announce a real and extensive improvement in the library. The meagre mention of a few books obtained through donation is not to be taken as quite in proportion to our ideal of what should be the accessions of the year 1891.

Since 1882, when a fund of $2000 was subscribed, no special appropriation has been made for library purposes except in the year 1886, when Mr. Brashear donated $117, the proceeds of a lecture.

The journals at present received cost the Society about $131 yearly.

Many of the reference-books on applied science now in our cases are to be regarded as excellent histories of almost forgotten methods and processes rather than modern treatises. It is true we can make a fair display every year of consular reports and other public documents; but there are unmistakable signs in many of these official documents that they never are consulted by those who frequent the library.

In the closet of the upper room are being stored the completed volumes of journals which, for lack of means, have not been bound for two years back. We have thus accumulated nearly two cubic yards of literature, which should now be bound in order to become accessible to members. More than 175 volumes are thus rendered wholly useless to the Society, as the danger of injury or loss renders it necessary that they should be locked up until they can be sent to the binder.

The needs of the library are plainly indicated, and are these:

1. That a sum should be provided for binding all journals as soon as the single volumes are completed.
2. A collection of standard books on metallurgy, electricity, engineering, chemistry, and other sciences should be procured.
3. Last, but not least, additional case-room should be provided in which to store the new books.

FRANCIS C. PHILLIPS,
CHARLES DAVIS.

The President then read his annual address.

ADDRESS OF THE RETIRING PRESIDENT.

The retiring president then briefly addressed the society. Referring to the growth of the society since its organization twelve years ago, he thought its success was ample evidence of its enduring vitality, and that it had in this comparatively brief period achieved a reputation in metallurgy and in engineering, art, particularly in regard to materials entering into construction, second to that of no similar society.

Referring to the subject of technical education the speaker regretted to observe in the curriculums of the Polytechnic schools less and less attention paid the classics, which he regarded as a serious mistake, for the labors of the engineer of the day required a better knowledge than ever before of the physical sciences, and that no profound or original researches could well be made in science—in any department—without at least a ground-work course in the classics, laid in the retentive years of youth. Upon a good foundation, even if not built upon for a number of years, work may be resumed and carried to any desired height. If we are to expect the engineer of the future to be simply a highly educated mechanic—for all time an employé—without the taste or desire for the better things which come of a more liberal culture, the existing methods would accomplish that object. Our own membership, as does that of some of the other large technical societies, disproves the assertion sometimes heard, that for the engineer it is time lost to study the classics, for those among us able to draw freely from the great store-house of science locked up in foreign tongues, bring forth the best tempered ideas, and must ever be referred to as authorities by those debarred from such privileges. Not only this, but with increasing years the leisurely enjoyment of the highest forms of even English literature must be forbidden the engineer.

It has appeared that our library has not grown as we could have wished, and I have thought that among the libraries of many of our members there are probably many books which are perhaps duplicates, sometimes bound journals, scientific journals, and others of interest for which they have no room. I have found that to be the case with several personal friends, indeed in my own house.

In the course of a few years we will get into a large library building, and we shall have the facilities for storing books, and I would like to draw the attention of the society to the question of preserving these things. We will take care of all books and pamphlets and place them on our shelves, where they will become valuable. Frequently these books, while not referred to by the owners, who are familiar with the contents, have an historical value.

Mr. Scaife for the Committee on Nomination of Officers for 1892, reported as follows:

For President, Alfred E. Hunt; Vice-President, P. Barnes, for one year; Charles Davis, for two years; Directors, Robert Munroe, G. W. G. Ferris; Secretary, R. N. Clark; Treasurer, A. E. Frost.

On motion, the president appointed Messrs. Wm. Metcalf and W. L. Scaife to act as tellers. The tellers collected the ballots, counted the same and reported the above-named members elected to the offices for which they were nominated.

President-elect Alfred E. Hunt was then escorted to the chair, and made the following address:

I will accept the position and will endeavor to do my best to fill it, not as creditably as has been done by my illustrious predecessors, but as well as I can, which is the best any one can promise. I shall ask the hearty co-operation, which I feel confident I shall receive, from all the members of the Society.

We have been, as has been said by our out-going President, an illustrious Society in several senses. We have had not only a hearing but an influence in the community and in Western Pennsylvania, and are a society that has done much good. I believe we have a field in the future which is open and which can be improved even beyond the extent to which it has been in the past. But that requires the co-operation of all the members of the Society. I would ask you all to give suggestions, and I promise to give such suggestions my careful consideration and steer between the ideas that shall be given, according to my best judgment, and with the views and advice of the Board of Directors of the Society.

There are quite a number of new suggestions that have been presented to me and to the Board of Directors of late, which it would be well for us to at least consider carefully before discarding them; to consider them also carefully before adopting them, because, as we know, every question has two sides.

One of the leading questions that has been presented is that our Society represents many different branches and has in its membership varied interests. Some of our members feel that their interests have not been considered in the past, that their own lines have not been properly attended to nor properly represented by papers and by books in the library, and in the discussions of the Society. Several ways to remedy this matter have been suggested, and in the course of the next few months these matters will, I trust, receive consideration by the Board, and some action afterwards will be taken by the Society.

Meanwhile any suggestions that can be made by any of you, as to the better furtherance of this subject of having all interests in the Society represented, will be received by me gratefully, and I know they will be by the incoming Board of Directors.

I again thank you for the courtesy of electing me.

The Secretary then read the minutes of the last regular meeting, which, on motion, were approved.

General discussion then being in order, the floor was given to Mr. Metcalf.

DISCUSSION.

MR. METCALF: A matter that has disturbed my mind for a number of years is this: I would like to know why it is that this society, which is certainly the strongest and ablest local society in the country, and I do not think it is very far second to any of the national societies, is never heard of outside of Pittsburg. It is never seen in the public prints, the technical journals never have any notice of us. Every little scrub club in the country has a half-page in every week, a notice of every meeting they hold, of all they do and all they say, but this great society, and it is a great society, is never noticed.

Somebody is at fault for that, and I think that is a subject which the new Board can take up.

As to the matter of having the diversified interests represented here, all those interests have to do is to bring the papers here and let us have a whack at them.

MR. CLARK: I will be very glad to receive any suggestions from Mr. Metcalf.

MR. METCALF: Every editor would be glad to get an abstract of the proceedings of the society.

MR. CLARK: It seems to me, taking up this subject as your incoming secretary, that it would be very easy for any of us who are going to read papers in the future to send an abstract to the secretary before it is read.

How can the secretary write an abstract of a paper on a subject of which, perhaps, he is perfectly ignorant, and send it to the technical papers. If the person writing a paper would send to the secretary a little abstract, the secretary would be only too glad to send that abstract around to all the technical journals; but it is pretty hard to ask the secretary, who only listens to the paper, to sit down and write out an abstract and send it off. In regard to the last paper read here, I could not have written an abstract of it, for the simple reason that I am not a bridge-engineer. If I should attempt that, I would do the author of the paper a great deal of harm, not to speak of myself. I think the best plan is to let the author write out the abstract, before reading the paper, and hand it to the society before the meeting.

MR. METCALF: That only touches the very smallest part of the difficulty. Nobody ever sees any notices in the papers that this engineering society of Western Pennsylvania had a meeting, and unless it is changed, there won't be any notice of our annual meeting to-night, any notice of the officers elected, or any notice whatever. The society is not known. I venture to say the secretary could, without very great labor, and it ought to be required, make an abstract of the meeting, that a paper was read by such and such a person, on such and such a subject, the discussion joined in by such and such people. I know that the editors of the technical journals would be glad to get such an abstract, and I believe that nine times out of ten the mere mention of the papers read before this society, by the gentlemen who read them, would lead to the editors of these papers themselves writing to the writer of the paper. But, if the editors do not know that the meetings are held, that such a society is in existence, how will they get them?

I do not think the secretary ought to try to abstract the paper. I think he ought to abstract the meetings in a small way; at least to get as much of a notice as the Philadelphia, Boston, Cleveland, and backwoods clubs. I think we are the biggest society in the country except the national societies, and we are hiding ourselves in our smoky city because we do not let our light shine out.

MR. SWENSSON: In addition to what Mr. Metcalf has said about giving an abstract of the paper, and the history of what we do at the meetings, I simply suggest that the secretary has to write out a copy of the minutes of the meeting, which is simply the history of what we do here.

MR. BRASHEAR: I would also suggest, that not only the abstract of the proceedings of the society should be sent out, but I think it is the duty of the secretary to send out to the different journals the papers entire, as printed and sent to our members. I read a little paper here that I thought was simply a part of my duty to this society, some years ago. I sent it to one journal, I remember, because I thought it would do some good to some poor amateur optician who did not know about the secrets of optics. That was reprinted in all the optical journals all over the world.

It did me good, and I hope it did the society good. They have reprinted it in full, and even reprinted it in journals across the water.

I think there are many papers read here of such value that they should be reprinted in the technical journals quite as well as the abstracts. I have always been proud to see, "Read before the Engineers' Society of Western Pennsylvania," attached to my papers, and I am always glad to read a paper before this society.

Mr. Roberts: Our poor secretaries are catching it, but we have had before us this question in various shapes. We have debated the question of joining with other societies in a syndicate of local engineering societies for the purpose of having our papers printed together. Individually I favored that, but I think it comes through this syndicate publication, that Mr. Metcalf sees the notices of these back-woods societies. We are simply "not in" that, but if we join this syndicate we would be. However, I think Mr. Brashear's idea would be excellent. The only difficulty would be in the individual work. It would be left to the members to forward their papers, but with the lectures our secretary has had he will make a stir this year.

Mr. Clark: I would like to ask the society for some authority, that the secretary be authorized to send to all the leading technical papers in England, on the Continent, the United States and Australia, copies of the papers as published. The secretary heretofore has never had authority to do anything of that kind since I have been a member of the board of directors. The board has never authorized it, and the late secretary has probably not wanted to take the responsibility of doing a thing which has never been done before. I ask the society for authority to send out these papers to the technical societies throughout the world, asking for their papers in return.

Mr. Harlow: In the last six months I have been unable to attend to the duties of the office very well, and the board would not accept my resignation six months ago, but in the matter of sending these papers we have a long list of correspondents with which we exchange. The advertisement of the meetings of this society is in some of the technical journals. I see it occasionally.

During the past year there has but one member given me an abstract of his paper. My idea was to get this abstract so as to get it to the papers on the day the paper was read.

MR. PAYNE: I think it is very unfortunate that this society should have no way of disseminating the information which is given at these meetings. Before I became a member of it, I took a great deal of pleasure in, and received a great deal of information from, the *Associated Journal* of the engineering societies, and I was very much surprised at never having seen anything in that of the Engineers' Society of Western Pennsylvania. It seems to me this is the proper time to take up the question of the advisability of our joining the Associated Engineering Society.

MR. SCAIFE: I should like to ask the librarian how many exchanges we have?

MR. PHILLIPS: There are twenty journals that come as exchanges. A great many are from societies, but not all. They all receive our *Transactions*.

MR. LANGLEY: It seems to me that the proposition to have abstracts of the papers forwarded to the secretary is a very wise one. It is the custom of a number of societies both here and in Europe to require absolutely an abstract of the paper to be in the secretary's hands before it is read to the society, and still more, before it can be in print. It serves two good purposes: it enables the secretary to learn something of the nature of the paper in advance of its publication, and it is the only feasible way to get the necessary abstract for publication.

MR. PHILLIPS then moved that the propriety of requiring from all members the submission of an abstract of their papers to the secretary before they are read be considered by the Board of Directors, with the request that they report on the same at the next meeting.

Carried.

MR. BRASHEAR: If we could only get our papers beforehand, our Board of Directors and our Programme Committee, who do not generally have more than one paper ahead, could look it over, and if there are no corrections they could get them out very quickly. For instance, if an abstract was handed in this evening it would be in shape to publish so that the paper could be read at the next meeting.

Mr. Davison: In connection with that, if the Board of Directors takes that matter in hand, they will have to be aided by the members of the society, because the case is worse than Mr. Brashear has mentioned, who has said that it is generally the case that we have only one paper ahead. I know of a number of instances where we have not even had one ahead. The subject of the paper was not known until two or three days before the meeting, and the Committee on Programme was hunting all over the town for it. The members of the Society will have to aid the Board of Directors in carrying out any law they may see fit to pass to bring about Mr. Langley's idea of what we should do. I think the idea is a very good one, but it will be hard to accomplish unless every one helps the Board of Directors to accomplish it.

Mr. Metcalf: My recollection of my service and experience in the board of this Society bears out what Mr. Davison has described. If this were a national society in numbers, with one or two thousand members and papers coming into the secretary in large numbers, larger than possible to be presented to the Society which meets but once or twice a year, and a great many read by title only with the members considering it of interest and importance to get their papers before the Society, to have them read in full and discussed at all, then such a law as Professor Langley suggests would be very important, but where it is difficult to get papers at all, what is the use of putting another burden on the committee which tries to get these papers?

However, the discussion has drifted off of the point. I did not refer to the papers. If the Secretary of this Society will notify the technical papers of our meetings, with a short abstract of the proceedings, including the title of the paper read, its author and who discussed it, it will arouse the interest of the members a little in trying to read papers, because they will know that at least there will be a published mention of them, with the probability that some paper or more than one paper will ask for a copy of it. That is really the proper way for an engineer to advertise himself, the true professional way, but the first thing is for the

Society to give these papers a fair abstract of our meetings, from a quarter to a half column. Give these papers just a general outline of the business done, and now and then you will see a paper published because an editor has been told of that paper and wants to publish it. Some editor has probably sent for it. I do not think you are going to improve either the quality or number of the papers by adding any more restrictions, because I know very well the difficulty of getting papers.

MR. PHILLIPS then moved that the Secretary be empowered to send copies of the proceedings to such papers or sources of publications as he saw fit.

Carried.

MR. HUNT: I am glad that certain things have been said here to-night, of interest to us all, by gentlemen whose professional reputation entitles what they have said to receive careful consideration, statements which younger men could not make with equal propriety. Both said it was a benefit to read papers before the Society. Mr. Brashear said it was a personal benefit and Mr. Metcalf said it was the way for the engineer to advertise himself.

If we consider that to be true, and I believe it to be thoroughly true, we should not have the trouble we had in the past of getting papers. If it is realized as a matter of interest and personal benefit to the members, to each professional man individually, to bring out these papers, the Programme Committee will not have to chase around as they have had to do in the past.

MR. BRASHEAR: There is one thing I would like to mention which might develop some interest in our meetings, and that is, that probably it would be well to take up an evening, or a little time at each of our meetings, by something in the way of a *conversazione*,—an acquaintance made by conversation. That is to say, occasionally there are things that come up in the daily life of each one of us, in the daily work, that are striking, that are important, that are interesting to each one, and a little time might be taken up by what is called, in some societies, "the good of the order."

Sometimes I have brought instruments here to exhibit, and I think such men as Mr. Metcalf and Prof. Langley, and I do not know who I would except, can give us notes that would be of personal benefit. And I will just say, in a very few words, something to illustrate that. We have just finished at our place the optical parts of an instrument that is to go to Breteuil, in France, the place where the standard metres are kept, which is to be used to investigate the errors in the metre in wave lengths of light.

Heretofore, the most critical studies have been made of the metre, and you know that we had a lecture on this subject by Prof. Mendenhall, in which we all were interested. The International Committee was not satisfied that this metre was a standard, or any of the metres that have been made, because they did not know absolutely its value in terms that are stable. The base of the metre was an uncertain base.

But now an American physicist has taken up the matter, and has calculated that the base of measurement should be a wavelength of light which should remain the same so long as the earth and the solar system remain in the present luminiferous ether. The question of investigating the matter by this method was taken up by Prof. Michaelson, who measured the velocity of light more accurately than Fizeau or any of the German or French physicists. We have made the parts of the apparatus to investigate this matter. The International Committee, I am a little proud to say, offered us the work without any solicitation, or any contract, or anything of that sort. We have just sent out our apparatus. It was required that each surface should be correct within at least one-twenty millionth of an inch; that was the least error allowed in each surface. The investigation is to commence in May and continue through the summer, and we will have, in a little while, the absolute value of the metre in terms of waves of light. I think sodium light will be taken as the basis. I may have the pleasure of describing this instrument before the Society.

The President reported the death of George J. Hicks, recently a member of the Society. On motion, the President was authorized to appoint a committee to report on the death of Mr. Hicks. Adjourned.

R. N. Clark,
Secretary.

FEBRUARY 16TH, 1892.

THE Society met in the parlors of the Academy of Science and Art on February 16, 1892.

One hundred and forty members and guests were present; among the latter were members of the Ladies' Committee on the Abatement of Smoke.

Alfred E. Hunt, president, in the chair. The minutes of the last annual meeting and of the regular January meeting were read and approved.

The secretary, R. N. Clark, read the following communication from the Board of Directors:

In regard to the minute of Mr. Phillips, moved and carried at the last regular meeting of the Society, to wit, "that the propriety of requiring from all members the submission of an abstract of their papers to the secretary before they are read, be considered by the Board of Directors, with the request that they report on the same at the next meeting," your board, after due consideration, feel that it is not expedient to make a rule insisting upon an abstract of all papers, but since it is very desirable to have such an abstract for publication your board recommends all authors of papers to submit such abstracts when possible.

The following gentlemen were elected members of the Society by ballot: F. E. Siebeninann, Chas. J. Farrar, James Scott, L. E. Jordan, Samuel H. Johnson, Dudley S. Liggett, Chas. M. Driver, Walter A. Ballard, Theodore Vandivort, Wm. Smith, George Lamb.

The president read a communication calling for a meeting of all interested in University Extension in Pittsburg, to be held at the First Presbyterian Church, February 26th.

The Banquet Committee reported that the committee had received $436 for 109 plates sold, that their expenses were $402; balance on hand $34. On motion of Mr. Metcalf the balance of $34 was ordered to be used for the benefit of the library, and the committee thanked for its successful work.

The paper of the evening was then read by Mr. Metcalf, which he prefaced as follows:

Mr. President, Ladies and Gentlemen: I need not make any apology for reading this paper for I was drawn into this matter by the fact of a very remarkable letter by a very remarkable gentleman appearing in one of our public journals a few weeks ago, which the editor sent to me to know what I thought about it. In a great hurry I dashed off a few lines saying what I thought of it at first sight. That led to a controversy in the public press, and not caring to enter into that kind of a discussion I felt that the subject was one of sufficient importance and interest to be brought before this Society. I felt that the Society would take an interest in it and discuss it thoroughly.

I also had the feeling that I preferred to have any opinions of mine tried before a jury of my peers as the legal right of everybody. Therefore I preferred to appear here where if I did say anything foolish I would be informed of it very promptly.

ON SMOKE.

In dealing with a subject of such universal interest as smoke, engineers will pardon me for an elementary statement of the conditions of combustion, which is necessary for the information of any general readers who may wish to read our discussion of the matter.

The combustion of coal involves two processes, first the conversion of the carbon to the condition of gas or vapor, and second the union of the carbon with the oxygen of the air. This union produces the intense heat so familiar to us all, and the product of combustion when complete is carbonic acid. In addition to this carbonic acid water is produced by the burning of any hydrogen that may be in the coal. Any sulphur in the coal is burned to sulphurous or sulphuric acid and any other combustibles are burned to their several oxides.

All of this seems very simple, and it is simple enough until we attack the practical part of it. The difficulty of mixing the gases to produce perfect combustion is so great as to be practically impossible; if we burn all of our carbon to carbonic acid we have inevitably a large excess of air going through our fire, and if we do not burn it all to carbonic acid in the few moments at our disposal in the furnace there is a deficiency of air and the excess of carbon is wasted in the form of carbonic oxide, half burned carbon or in vaporized carbon which is thrown off with great rapidity in the dense black vapor which we call smoke.

IMPOSSIBLE TO BURN IT ALL.

In the intense heat of a fiercely burning fire our bituminous coal is vaporized with such great rapidity that it is impossible practically to burn it all before it flies to the chimney and passes beyond the reach of combustion, but much may be done by steady mechanical firing in small quantities at a time to reduce the smoke nuisance where the most intense heat is not necessary and where other and more important matters do not make other methods of firing imperative. A flame of perfect combustion is not white; it is blue or a bluish-white, somewhat of the color of the arc light. Such a flame is not a good radiator of heat, and is therefore not so efficient for heating or melting purposes, as the cooler white flame of imperfect combustion which we know as the radiating flame. The carbonic acid non-radiating flame is excessively destructive; its cutting power on anything with which it comes in contact is amazing, when we see a furnace of the best refractories cut down and ruined in a few hours which would endure a steel melting heat for many weeks or months if it were subjected to a white radiating flame only.

Many of us know to our heavy cost and intense disgust how easily a careless or ignorant man can so burn up in a short time hundreds of dollars worth of the highest refractories and lay the whole plant idle for repairs. We cannot produce the soft, white, radiating flame without some smoke, because we cannot attain an exact balance of the gases, and therefore to maintain such a flame we must have a deficit of air, a surplus of carbon and some smoke.

THE DICKEY BIRD'S SONG.

In operating a reverberatory furnace for melting, puddling or heating the smallest loss from the perfect combustion flame is caused by the destruction of the furnace; the furnace contains in its charge of iron or steel something of far greater value than the firebricks, and the terrible oxidizing power of that flame can only be realized by those who have to wonder where their iron has gone, and to wonder why their losses in operating are so great and their profits are so small. Meanwhile the gentle "dickey bird" sits aloft on the blast-furnace and whistles to himself softly as his neighbor's losses come up the elevator in the shape of cheap ore. People call it slag; the bird sings ever so gently "no smoke," "use coke." There is no smoke from a blast-furnace because coke is used, but the vapors of a blast-furnace are far more deadly than smoke, only they are white and so they do not count.

A beautiful illustration of the effects of the two flames we have been discussing, may be seen by watching a puddler's operations. When he has scrap-balls to cut down to make bottom, he rushes up his fire, pulls his damper wide open and draws in a great surplus of air. In a few minutes the little ends of the scrap glow white, and presently little molten globules may be seen dropping off here and there; they increase in number and in size until little streams are running down the pile; the whole pile is dripping, and fades away to a last little lump, which the puddler pulls out because it would be a waste of time to cut it down. Melted iron? Not a drop; his furnace is never hot enough to melt wrought-iron; it is melted oxide of iron; literally, the iron burned up.

Clear Skies are Costly.

The bottom finished and set, he goes on with the well-known operations of melting and boiling until his iron comes to nature; and now how different are his motions; the damper is adjusted to a nicety, a little more fuel is added, until there is a surplus of carbon and a deficit of air; he gathers up the little grains and sticks them together into little balls, and these into larger balls, turning them repeatedly, keeping them covered with slag to prevent oxidation from any possible surplus of air or carbonic acid; and so, quickly and carefully, he makes up the balls to the right size and rushes them out of his furnace to the squeezers or hammer.

Any oxidation now means loss of iron, and consequent loss of wages and hard toil wasted.

Don't let him make any smoke, though; what matter if his well-earned wages do burn up, provided we may have clear skies, and I was going to add, pure air, but we will come to that question later.

With natural gas, the common puddling and heating furnaces do not make much smoke, but the awful waste of gas is a sin for which Pittsburg is paying dearly now, and must continue to pay for many years to come.

Did we have five years of comparative smokeless puddling and heating in reverberatories? Then we used up thirty years' supply of the precious gas in doing it; and some of us are still shooting it up our stacks in the same happy-go-lucky devil-may-care style.

When we come to the regenerative furnaces, one would say, naturally, here at least there is no excuse for smoke; many of us thought so, and many of us tried it. I have seen hundreds of valuable crucibles cut down, and the furnaces cut down and destroyed, in the beautiful blue-white perfect-combustion flame, and yet the steel in the crucibles was not melted properly.

TONS OF IRON WASTED.

Tons upon tons of iron and steel have been wasted away in the same beautiful flame, and yet the masses were not heated through, nor in condition to be worked. Now, the invariable rule is, you must have a smoky stack, and the evidence of a well-run plant is that the stacks do smoke. It is a little odd, at first sight, that in these furnaces natural gas makes a little denser smoke than producer-gas, but there is nothing singular about it when we reflect that it contains about four times as much carbon as producer-gas.

The most persistent smoker is the boiler, and the reason is obvious; there are no hot walls there to radiate back the heat, or to aid combustion. The very object of the boiler is to destroy the fire and rob it of its heat as quickly as possible; therefore, every particle of gas that comes unburned into contact with the boiler-shell must float up the stack unburned, surplus of air or not, and add to the volume of smoke. The ordinary boiler-fire goes through three stages: the freshly-fed, when it pours out dense volumes of the blackest smoke, and carries what heat there may be up the stack, and there is very little steam raised; the good-burning stage, when there is a glorious fire, the boiler steams tremendously, and at dangerous speed, and there is but little smoke; the perfect-combustion stage, when there is a beautiful clear fire, no smoke, and the surplus air is rushing along and carrying off so much heat that the water begins to rise and the steam to fall. Then, the weary fireman opens his doors, rattles out his ashes, shovels in another cart-load of coal, shuts up his doors and lets her smoke, while he sits down to wipe off the sweat, and, if he is a sensible fellow, to smoke a little himself.

Why Producer-Gas is not Used.

Why not use producer- or water-gas, and prevent the smoke, even if there is a little excess of air; there is no glowing iron nor delicate steel to cut down here? It has been tried in England, Scotland, Germany, and even among the patient Dutch (how a Dutchwoman does hate smoke and soot!). They all gave it up, Dutch, and all; and even here, in Pittsburg, we tried it at an expense of thousands of dollars, and we thought we had it, but we were all mistaken.

Is there no remedy, then? Yes, a partial one. The first stage can be eliminated entirely by good automatic stokers; but then the second stage of fierce firing cannot be reached, and, on the whole, probably more boilers would be required. That was the experience, thirty years ago, with the Meisner grate—probably as good a one as any of the more modern stokers; but that, in the end, would not be a hardship, because the more moderate and even firing would be so much easier on the boilers that the reduced repair-bills would probably more than balance the interest on the increased cost. But these automatic stokers do smoke, just a little, and they keep it up, so that our nuisance is only lessened, not abated.

With natural gas, boilers may be fired smokelessly when gas is plenty. The favorite way with gas companies is, or was, to furnish two or three pounds of gas, a whole lot of squirt-guns called air-mixers, a lighted match, and then let her rip. Oh! how delighted they are, or were, with their perfect combustion and their smokeless fires. And oh! how they swore when they saw their big millions of feet consumed and their little piles of dollars received. There are, or were, no meters under the boilers.

A SIGHT TO BEHOLD.

The boilers under my charge were fired that way at first, and the way they roared, and rattled, and vibrated, and perfect-combusted and didn't make steam was a sight to behold once in a life-time.

And the way I shook and trembled with fear while that was going on was an experience to be endured not more than once in a life.

After an expenditure of much time, some thought, and considerable money, a partially regenerative fire was adopted, by which, with from two and a half to three ounces of gas, we can keep steam up nicely, but singularly, when the gas is very low, when every atom seems necessary, then, to keep up steam at all, we must exclude any surplus air so thoroughly, that now our boiler-stacks smoke a little—only a little; still, they do smoke; and so the evidence of the best practice again is a little smoke in the stack.

The next thing to consider is, what do we lose by smoke?

There are smoke-consuming devices advertised claiming savings in fuel of from 10 per cent. to 25 per cent.

The best authorities I know of give the extreme of loss from smoke as 5 per cent., and the mean loss from average firing as 2 per cent.; therefore, if the devices mentioned do save from 10 per cent. to 25 per cent. of fuel, they are misnamed; instead of being called smoke-consumers they should be named heat-savers. That such saving over ordinary wasteful methods can be made there is no doubt, and if in doing so they save the 5 per cent. that goes as smoke and prevent the smoke, so much the better, provided that in furnace-firing they do not burn up ten times this value in iron, or steel, or valuable refractories, or all three together.

Some Wastes of Heat.

Some years ago I had the honor to read before this Society a paper on "Some Wastes of Heat," showing that an annual loss in this county of some $1,100,000 was fairly divided between wasted fuel and burned iron and steel. Although many improvements have been made since that time, it is probable, judging by the appearance of many stacks, that the same gross loss is going on in the county to-day. That money would be well worth saving, and it could be and would be saved if our proprietors could be made to believe that there is such a loss. Through this want of belief the loss continues.

It is certain that the best and most economical appliances will produce a minimum of waste and a minimum of smoke, but not an entire absence of smoke.

Can smoke be prevented by the use of coke? Undoubtedly, if coke can be made without smoke. It would be hard to point out anything dirtier or nastier in the way of a smoke nuisance than a coke oven.

But coke is made out in the country, not in the city.

Well, is not the country bigger than the city? with more people than the city? Is is not far more beautiful than the city, and of much greater value than the city? What is art but a mean imitation of nature; and are we to daub and smear the whole face of nature to save a few puny works of art?

Let us have coke by all means, but first let us have it made without smoke; let us be at least decent to our neighbors while we are being kind to ourselves.

Looking Backward.

Here I became dreamy and went back to the days of my youth, when for a vacation I used to love a jaunt to our beautiful mountains. It was a charming autumn evening, the place, the summit of Chestnut Ridge; the scene, that lovely land of hill and dale that is watered by the sparkling Loyalhanah, the Youghiogheny and the Monongahela. Away in the west rises a cloud of dark mist, locating dear old Pittsburg and home.

The mist spreads, and slowly envelops hill after hill, and farm after farm, clear to the base of the mountain. A few dull red flames appear here and there, they increase in numbers to hundreds, and then to thousands and thousands, and the whole country is covered with Parton's Pittsburg many times enlarged.

In the forest near us, a spectre slowly rises and shapes itself into the form of a dear old German lady come back from spirit-land to have another look at the scenes of her former life; she looks long and earnestly, and an expression of unutterable sadness comes over her face; the "little dickey bird" in the tree close by attempts a little song to cheer her up, but the deadly fumes choke him and he droops and dies.

The spectre takes one more look, whispers sadly and softly:

"So it never waas," and vanishes forever.

That's coke.

Another view: It is a clear, bright Sunday afternoon; we are up on one of the hills that surround the city, enjoying the fresh air and admiring one of the most beautiful inland views in the world. Five o'clock comes, and little curls of light-brown smoke are seen here and there; they increase in numbers, and now there are thousands of them; a mist spreads over the scene and grows thicker and blacker, until by six o'clock, rivers, cities, and all, have disappeared; there is one unbroken lake, from hill to hill, of smoke, nothing but smoke.

That's tea.

A Remedy Suggested.

Let us have no more tea then, nor let the good housewives burn coke; or better still, let us apply the Sunday laws to the metres, so that they shall not be allowed to work on Sunday. Then we can have tea, clean skies.

If what has been said is correct it may be a fair conclusion that some restrictive legislation wisely planned against excessive smoking might be good for the community; on the other hand, it would be equally fair to say that prohibitive legislation would be sure to defeat itself.

Naturally, one would ask here, has science reached its limits, can no more improvements be made? By no means; everything is crude now when we study the possibilities of the future.

I will indicate two matters for our younger and more active members to think over: first, sift the oxygen of the air away from the nitrogen; it is only a mechanical mixture of two gases, and the problem is not insuperable; then with pure oxygen at command, setting aside all other wonders that may be worked, the second problem ought to be more solvable, viz., the direct conversion of the heat of fuel into electricity; that accomplished the converse would follow as a matter of course, the conversion of electricity into heat; then we should have done with smoke, dirt, ashes, gases and all. But there is a health association after the smokers and we must consider the question of the health of the community as it is affected by smoke. I assert that there is nothing particularly unhealthy about smoke; on the contrary, it may mitigate other and worse evils. A reference to statistics will show that this city is not particularly unhealthy, but that, on the reverse, it enjoys a rather low death-rate.

SMOKE IS NOT UNHEALTHY.

A contemplation of the beautifully clear air of our Atlantic seaboard cities, the most beautiful cities in the world, is apt to fill a Pittsburger's heart with envy, but in spite of clear air and balmy sea-breezes those cities are not healthier than ours is. Their anthracite coal gives off as much carbonic acid, carbonic oxide, sulphurous acids and other poisons as it is possible to get from our bituminous coal. Who that has ever inhaled the burning, biting fumes of anthracite would not infinitely rather have a dose of our blackest smoke. Woe to the weak pulmonary organs that breathe anthracite fumes; there is no unctuous protecting coat of soft, pure carbon to save them from torture and destruction.

A few years ago none but the most robust could hope to live in our windy, dusty lake cities. Why is it that we hear so little of those terrors nowadays? There is no difference except that we have sent them lots of coal, plenty of smoke and have painted and protected their bronchia and appurtenances with a generous coat of our all-protecting carbon. All returning travellers from Colorado are justly in ecstacies over the wonderful scenery and the marvellous air, yet wise old doctors out there who know, tell us that there is not a healthy woman in Colorado, and I heard of one old rascal who said there was not a good-natured woman there either. Healthy women, where are they to be found if not in Pittsburg? Noble women, beautiful women, women to be proud of the world over, and lovelier girls, nowhere. It is for them that our mills roar and our hammers thunder, and for them they will continue to rattle and rumble through all time.

But is it not a pity to mar so much loveliness with this horrid soot? Look in the picture galleries at the beauties of a century ago; they put great ugly patches of black plaster on their faces to enhance their beauty, and will any young man here dare assert that one of our girls with a flake of soot on her fair nose is not infinitely prettier and sweeter than one of those old-time beauties?

And, finally, young man, if you should be so gallant as to remove one of those soot flakes it would not taste near as badly as a piece of sticking plaster, nor would it look quite as badly on your mustache. It would not be quite so strong in evidence you know.

DISCUSSION.

PRESIDENT HUNT: Certainly some one here will be ready to take up the cudgels against smoke now that Mr. Metcalf has brought the other side to our view in favor of it. I will call upon Mr. Porter to open the discussion.

MR. H. K. PORTER: In the first place allow me to congratulate the Society on the delightful showing that you make in your annual reports and in the communications of interest that there are in connection with scientific matters of this nature that you are discussing and taking hold of, and that you have given us a report of to-night in the records of the last meeting.

And allow me to express thanks, also, from many parties who are strangers in your meeting at the courtesy shown and extended in inviting us to be present and to participate in the discussion.

I was invited to render a special service here to-night, and that was to present for the ladies of the Ladies' Health Association a letter that has been received by them from Mr. Wilson King, a much respected citizen of Pittsburg, who has acted as our representative for a great many years in the city of Birmingham, England. I believe the name of Birmingham is pertinent in connection with this subject of smoke, and if we can have a calm and deliberate statement from a gentleman of the character and standing of Mr. King in regard to the atmosphere of Birmingham, where the same fuel is used now that was used a great many years ago, and we can see there has been a decided improvement, then we can feel that the cheerful words and the cheerful side that was brought out in Mr. Metcalf's paper gives us hope even here.

I am sure that there is not a single citizen of Pittsburg who remembers the five or six years of immunity that we have had from this dreadful curse but is grateful for that experience. I think there is no one of us who has not enjoyed and delighted in the beauty of our atmosphere, and the beauty of the natural scenery that God has given us here in Western Pennsylvania without this fearful pall of smoke. And I have not learned and I do not believe that any citizen of Pittsburg has learned that there have been serious .and frightful losses from a commercial point of view from having that abatement of the nuisance. I do not believe there is any one of us, no matter how much we may feel the necessity for making smoke, but would be very grateful if we could see a way by which equally good or better results might be obtained than to have them in connection with the smoke, and I do not suppose there is any one who has any desire to ruin the interests of Pittsburg, any one who has a desire to take bread out of the mouths of the workingmen, any one who wishes to lessen the profits of the manufacturer, or.anything of that kind in connection with this desire of freedom from smoke.

It seems to me that the extra expense involved in any appliances which may go to reduce this evil is a small consideration if we can only be relieved of the nuisance of the smoke. I think we shall all be grateful, because there is such a world of cheer in this letter that Mr. Wilson King has sent, that the ladies have wished to have it presented here this evening.

LETTER FROM WILSON KING, ESQ.

"Birmingham, a manufacturing town of half a million inhabitants, and using no fuel but bituminous coal, is comparatively clean. That is to say, it is a great deal cleaner than London, or Chicago, or Cincinnati, or than Pittsburg was before the introduction of natural gas. The coal used is not so soft as Pittsburg coal, and does not, perhaps, make quite so much smoke; but, on the other hand, the atmosphere is more humid and heavy than it is at home, and has a tendency to keep the smoke down.

"The reasons that Birmingham is thus comparatively clean are doubtless the Smoke-consuming Act and the public spirit which sees that it is enforced. Of course, no town using bituminous coal can be as smokeless, and, consequently, as clean, as one using gas or hard coal, because of the very great amount of smoke made by the dwelling-houses with which no law as yet interferes; but most of the factories can be prevented from emitting much smoke, and by doing this the authorities have made Birmingham a far pleasanter place to live in than it was formerly.

"The law is as follows:

PREVENTION OF SMOKE.

"First: Every furnace or fireplace used within the city in the working of engines by steam, or in any mill, factory, dye-house, brewery, bake-house, gas-works, or in any manufactory whatsoever (though a steam-engine be not used therein), and every steam-engine used within the city, shall be so constructed as to consume the smoke arising from the combustibles used in such furnace, fireplace or steam-engine.

"Second: If any such furnace, fireplace or steam-engine, so constructed as aforesaid, be so negligently used as not to consume the smoke arising from the combustibles used therein, the owner or occupier of the premises wherein the same is used shall be liable to a penalty not exceeding five pounds, and to a further penalty not exceeding forty shillings for any day during any part of which such furnace, fireplace or steam-engine shall be so used, after fourteen days' notice has been served by the corporation on the owner or occupier of the premises wherein the same shall be so used, to remedy or discontinue the same.

"Provided, that the words "consume the smoke" shall not be held in all cases to mean consume all the smoke; and that a court of summary jurisdiction may decline to convict, or may remit the penalties enacted by this section, if the court is of opinion that the furnace, fire-place, or steam-engine has been so constructed or altered as to consume, as far as possible, all the smoke arising therefrom, and has been carefully attended to, and has consumed, as far as possible, the smoke arising therefrom.

"This law does not apply to locomotive engines, but, as a matter of fact, they make very little smoke, either in town or country, and if one were to habitually go through this city belching out horrid masses of sooty smoke, such as I have constantly seen issuing from the funnels of steamboats and locomotives in Pittsburg, I think it would produce a riot. It is only meek Americans who put up with such tyrannous outrages.

"It is the business of health inspectors to report any breaches of this law; the police are encouraged to do the same, and it is regarded as public-spirited on the part of any citizen to do the same.

"In 1889, the last year for which I have an official report, the number of manufacturers reported for the emission of dense smoke was 313. Of these, 172 were cautioned, 94 convicted and fined, and the rest were able to show that they were justified in producing the amount of smoke they did. The law has thus been shown to be effective, but it must be carefully and tirelessly enforced. At least one case has come to my knowledge of a manufacturer who believed he would rather pay his fines than go to the expense and trouble of preventing the nuisance; but, by repeating the summons against him as often as was legally possible, and by enforcing his personal attendance before the magistrates, he was induced to believe that the expenditure of time and temper, as well as money, was too great, so that he gave in and complied with the law.

"In some classes of manufacturing it seems impossible, at present, to prevent the making of much smoke; but, that such is the fact must, in each case, be proved to the satisfaction of the authorities.

"In many cases, the consumption of smoke is a decided saving. Some manufacturers convert their fuel into gas before using it under the boilers. I know a good number of important works where this is done and very little smoke made.

"In the boiler-rooms of an electric lighting company I saw an automatic mechanical stoker at work the other day. The finest, cheapest slack only is used. It is fed in from above, and as it falls it is struck and scattered all over the fire in the furnace and wholly consumed. On the other hand, the plant must be expensive."

A gentleman who has taken an active interest in matters affecting the health and welfare of the people of Birmingham tells me that many factories now use Henderson's centrifugal mechanical stoker, and find it fairly satisfactory.

The ladies gave me also a letter, which I will read, if it is your wish, from Chicago, from an agent of the Society in Chicago, but I will say, as it is only fair to say in regard to this matter, the gentleman has but recently been appointed to the position as consulting engineer of the Society in Chicago. Mr. White has recently gone from Dunkirk to Chicago to take the position of Consulting Engineer of the Society for the Prevention of Smoke. The names of the Advisory Board include a very large number of some of the best-known citizens of Chicago.

Mr. White writes: " With regard to puddling-furnaces I will say nothing whatever, for the reason that I have no information upon them based upon actual experience. With regard to steam-boilers I can say that it is perfectly possible, and entirely within the limits of the practical considerations of steam users, and of engineers in charge of steam-plants, to perform the work which such boilers are called upon to do, to do it with ordinary soft coal, of any quality, from the slack to the very best; to perform this work without production of smoke in such quantity that the most critical would not pronounce it a nuisance; and it is possible to do these things within the limits of any reasonable expense to the owner of any boiler-plant.

" I am aware that this is a mere statement, but in every city where soft coal is used, there are furnaces of reasonable first cost doing the work of burning soft coal smokelessly, and with economy fully equal to any which may be obtained with the ordinary appliances, and no attention given to the question of smoke. These furnaces have, many of them, been in use for long periods of time, and any engineer or boiler-owner who sees fit to take the trouble to examine candidly what has been done in his own vicinity can ascertain the foundation on which these statements rest.

"You say, yourself, that there are nearly 200 places in Pittsburg where satisfactory results are obtained. There is no rational reason why 200 may not be made 400, and 400 be made 4000, by the application of intelligent and practical common sense in the conditions involved in each and every case.

"The Society for the Prevention of Smoke, in Chicago, embraces among its promoters foremost names among its manufacturers, and those as vitally interested from a business point of view, as it is possible for any one to be interested in the city of Pittsburg. These men are giving their money and their time, as well as their influence, to the work of the society, believing that the ends sought are worth the expenditure of time and money, and that such expenditure, if wisely handled, will result in very large practical good.

"Chemical analyses show plainly that Pittsburg coals have heating power equal to evaporating between 13 and 14 pounds of water per pound of coal, and the same analyses show that the average Illinois coal, such as is used in the markets of Chicago or St. Louis, under the same conditions, will evaporate in the vicinity of 10 pounds of water per pound of coal.

"It will not be disputed that in a general way the higher the heating power of bituminous coal the easier will be the problem of its smokeless combustion. Therefore, what is done in scores of instances in Chicago, with entire commercial success, can without question be done by Pittsburg, with similar commercial success.

"It is impossible for me at this distance to enter into or give advice with regard to methods or means which should be employed to reach this result. The work in Chicago is being taken up step by step. The facts relating to each specific case are ascertained, and this being done, it becomes a matter of comparative ease to point out with a great degree of certainty what should be done for each such case. As the work goes on, and the field is more and more covered, it will undoubtedly tend to propagate itself by mere force of example, and thus expedite the work, which at first must be, of necessity, slow.

"C. F. WHITE,
Consulting Engineer."

Mr. White has not thrust himself upon us. He has been invited to express an opinion, and he has done so. It seems to me that the ladies who have this matter so much at heart have a right to feel that this is not a matter of sentiment, or that it is not practical. If there is anything practical, it is the question of our homes. The desire of all is to lessen the toil and the painful experiences of the women of the city of Pittsburg. Those years here of freedom from smoke were to me a delight, to see the women saved from degrading work that so many of them all through this city used to do. You never could travel through these streets without seeing them scrubbing their sidewalks, so that they could not devote their attention to something else. It seems to me perfectly feasible if we have the earnest desire in this direction. There are places in the central part of our city, along the river banks, among the mills, where a great deal can be done; and much has been done to lessen the nuisance; but much can be done in the heart of the city, in business blocks, in large establishments, wholesale dry-goods houses, and things of that kind, right down in the retail district, to lessen this nuisance, if we only have the desire to do it. If the gentlemen of scientific abilities connected with the Engineers' Society will only see that the end of science has not been reached, and that there are higher attainments to which we can go, and will devote some of their energy in that direction, they can greatly bless this city, and would receive the lasting gratitude of its citizens.

MR. W. E. KOCH: I have had a pretty long experience in smoke and its remedies. The last speaker (Mr. Porter) has mentioned Birmingham; I served part of my apprenticeship there over twenty years ago and I know the town well. In those days there were multitudes of small works, and they made lots of smoke, but times are changed and the old ironworkers are gone—hundreds of men have left the town or gone into other trades. Steel has taken the place of iron; much of the steel is made in Scotland and elsewhere and is sent to Birmingham to be worked over. Steel works are cleaner than iron works, and so the Birmingham of to-day is utterly different to the "old Brum" of twenty years ago. The puddling furnaces are gone and the small manufactories that made the smoke. In 1874 there was a great fuss about fog in London, and the late Dr. Percy, the metallurgist, was experimenting as to the cause and prevention of fog for the benefit of the British House of Commons. It was then found that the smoke-makers might be put in three classes: First, where smoke could certainly be prevented, *e.g.*, cotton mills, woollen mills, engineers' shops, printing houses, bakeries, etc., where boilers were running regularly and doing regular work from six to six. Where the pull on the boiler was constant and the same amount of steam was required from hour to hour. Second, where the consumption of steam is irregular and where a reducing flame is necessary, as in heating a puddling furnace. Where, in short, it is impossible to avoid making smoke at certain or rather uncertain times—for example, in a steel mill if all goes well, after the fires are once lighted, it is possible to consume most of the smoke from the boilers, but if there be a stoppage while the boilers are making ninety pounds of steam you must reduce fires, then when the call comes for more steam you cannot help making a good deal of smoke. In puddling and heating furnaces, as Mr. Metcalf has pointed out there must be smoke or waste of product will result. The third class, and the one which makes the most smoke and does not try to prevent it, is the house chimney. Some of the London fogs occur on Sunday when no factory chimney is busy. The London housewife makes more smoke to the square foot of grate than any factory. I think that smoke-consuming, like charity, should begin at home.

The late Dr. Percy forced London fog through a layer of cotton batting and found that the batting became wringing wet with a yellow oil; this was hydro-carbon formed by distilling coal up the chimneys in place of burning it on the grates. A great deal of smoke in mills can be prevented, but you cannot stop smoke in ironworks.

Pittsburg will become comparatively smokeless when the ironworks are changed to steel works or vanish altogether. We do our best to save fuel, and that means prevention of smoke; in our mill we use the Walker smoke consumer, and we really do not make dense black smoke; even when the boilers run irregularly it is not so bad as when we did not use any smoke consumer at all.

In Glasgow, where there is time limit (three minutes I think) during which you may make smoke at one time, we tried many devices on our puddling furnaces, but the only complete remedy was changing to steel. In such towns as Manchester more can be done in the way of smoke prevention, for though there are plenty of factories there are no iron works or steel works, either of much account as smoke producers, nor are there as yet any steamers, which contribute liberally to the fogs of London and Glasgow, not to mention Liverpool.

PROF. PHILLIPS: I think the most important lesson Mr. Metcalf has taught us this evening is one that perhaps he did not intend to convey to us. He tells us that one of the main difficulties in the way of smoke consumption will be that although smoke may be burned, the excess of air under the boiler will tend to burn the iron, the excess of air producing a higher temperature will act upon the refractory materials, firebrick linings and so on. The real question is then how to construct so as to prevent that evil. That is really a more serious question now than the question of how to burn smoke.

I hardly like to speak on the other side after hearing such an exhaustive paper from Mr. Metcalf, but I happened to receive, before I knew anything about this paper, a letter from one of the officers connected with the Chicago Board of Health. They have an office there devoted to the subject of smoke consumption, an Inspector of smoke and four assistants. On a recent visit to Chicago I spent a day looking at smoke consumers in use under boilers in many of the large buildings down in the heart of the city. I was very much impressed with the fact that it was the general conviction prevailing among the owners of these large buildings that there need be no smoke; and when I asked them any question about the smoke-consuming devices they at once said, "of course do away with smoke," and the very idea of asking any question seemed to convey the implication that they were not doing their full duty.

This impression certainly prevails in Chicago. "We must consume our smoke and we are doing it, and if anybody says we are not doing it we must assure that person he is wrong." I will read a very short extract from a letter I have received. I inquired of the Smoke Inspector if the impressions I had received were correct. This letter is from one of the Assistant Inspectors of the office. "We have had, I may say, remarkable success in the past year, over fifty per cent. of the violations being abated and the public, as a rule, are co-operating with this department, and the time is not far distant when this city will have solved the smoke problem. Where bituminous coal is universally used, as in this city, it is impossible to abate the smoke nuisance entirely, as the smoke device, the furnaces on which we depend to abate the nuisance, are inoperative when the fires are started, as all devices are dependent on the steam and heat of the combustion chamber to prevent the smoke. Our only alternative if we have to abate the nuisance entirely during the day will be to compel steam users to start their fires with smokeless fuel."

I found that there was much comment upon the improvement due to the use of smoke consumers. While in Chicago I had an opportunity to get some data as to the actual gain resulting from their use. Some of the devices do not affect fuel bills. Others involve a saving of fuel. I am not here to advocate any particular smoke consumer so it will not be worth while to mention the name of any device. It is said that during the month of October before a certain smoke consumer was used the quantity of coal used under a large boiler in a business block in Chicago was 76,700 pounds. During November, after introducing a smoke consumer and when more heat was necessary and more steam used, the amount was 53,000. During December it was only 47,000 pounds. No one could claim that such a saving will inevitably follow the introduction of smoke consumers. It was no doubt largely due to the additional care needed in using a smoke consumer.

Now, as to the subject of smoke consumers in iron-working: With the greatest respect for the gentlemen who have spoken, who are better informed than I am, I think that successful trials are being made in Pittsburg to-day in which heating furnaces and puddling furnaces are run with smoke consumers, and I heard a very intelligent mill-manager say only a few days ago, in corroboration of a statement he had made seven months before, that he considered the device being used at his furnaces very successful, and moreover, that the action upon the lining and upon the iron was in no way deleterious. The fact that smoke consumers are now being used for heating and puddling furnaces, and that they have continued to give satisfaction at a single mill, shows that important progress has been made towards obviating smoke in iron-working. No smoke was visible above these chimneys.

Mr. Metcalf refers to soot of incompletely burnt soft coal as preferable to the fumes of well-burnt anthracite. Yet there is one thing to be considered. The sulphur fumes or sulphurous acid that is carried off from the anthracite coal fire are very disagreeable, but sulphur is carried off with the particles of soot in very considerable quantity. If you take the trouble to burn a little soot, you will find sulphur still there ready to be burned out. Smoke carries the unburnt particles of sulphur with it, and when it falls it tends to rust iron and disintegrate limestone. This action goes on slowly, but may be worse in the end than that of gaseous sulphurous acid.

PRESIDENT HUNT: Dr. Sutton, what do you think of that?

DR. SUTTON: The subject before the Society for discussion, Mr. President, is in the hands of the people who make smoke. They understand the subject better. They make the article. Those of us who do not make it, who have to deal with it, have more difficulty probably to discuss it, but we certainly feel the effects of the smoke quite as much as anybody else.

There is no doubt at all that in England they have controlled the smoke nuisance to a very great extent. It was my privilege, nine years ago, to live in Birmingham for several months. I can substantiate, therefore, what Mr. King has said in his letter which was read by Mr. Porter, and I can assure you that if we had as good control of the smoke in Pittsburg as they have in Birmingham, few would complain. They have controlled also the smoke in London. It is true, London is a city of fogs, but when fog and smoke mix, it makes dense darkness.

Pittsburg is a city of fog, and when the smoke and fog are mingled together it makes great darkness. Now, it is a singular thing that in London at the present time there is likely to be a retrograde movement in regard to this smoke matter. Lying here on the mantel is a journal, the *British Medical Journal*, published in London, of the date of January 2, 1892. An article in it states that the former laws with reference to the consumption of smoke, the prevention of smoke in London, are about to be annulled, and the editor says—

(Here Dr. Sutton read an extract from the magazine mentioned.)

The bills have been defeated, and they are going back into the smoke. We are going back into the smoke. We had four or five years of wonderful cleanliness for Pittsburg, and we have all had a taste of knowing what it is to be clean. We all felt better, we all looked better, we all were better. But we are back into the smoke. It is growing worse day by day. It is grinding the lives out of the housewives of our cities, it is increasing the expense of living. It is destroying furniture, it is destroying fine art, it is destroying pictures, making people regret that they have spent their money for good things.

We all know that in past years Pittsburg has not been up to the mark in her institutions; she has not been up to the mark as a city of refinement; she has not been up to the mark as a city which commanded the talents in the professions at large. It is a fact, that as our ministers have become noticeable here they have been called and taken to other cities. The only professional men who have stayed here, have been lawyers.

Twenty-five years ago, when I came to the city of Pittsburg, it was the rule that when a man had made a fortune he went elsewhere to spend it.

If we are going to have a smoky city again, we must expect to have dirty parks, a dirty court-house, dirty private residences, offices, and street-cars.

Now it seems to me that we must look at this thing in more ways than one; that there are more things to be done here in western Pennsylvania than simply puddling iron. If it is so that we cannot make iron without dirt, I am perfectly willing to make iron. We will take the dirt, but I do not see any necessity for a printing-office pouring out enough smoke from the top of their chimney to ruin a whole ward. I do not see any necessity for a wholesale dry-goods house making enough smoke to fill Fifth Avenue for two blocks. I do not see any use of a common bakery that is making doughnuts ruining half a ward with its smoke. I do not see any use of lighting a city with electricity when the light plants are started after dinner, and before five o'clock in the evening the whole city is so dark with the smoke from their boilers that all the electricity they make won't light it.

Now, they tell us this smoke is healthy. I will admit that carbon is a great absorbent of noxious gases. I will admit that bacteria cannot grow where there is so much carbon and sulphur, but it seems to me also that a man cannot grow there and be healthy. Take an atmosphere in which plants will not grow, an atmosphere in which trees will not grow, and it is not a good atmosphere for man, and such is the atmosphere of the city of Pittsburg.

I believe that that principle which teaches us to put up with the necessities of our neighbor is the right principle. I think we all ought to submit and be willing to submit to those things which are positive necessities, but where they are not, they should be crushed.

MR. A. DEMPSTER: Mr. President, I do not want to occupy the time of this meeting in discussing this question to-night, as it is getting late, and as there are others here who have had more experience, and more information on the subject than I.

However, I have looked out of the window of my office many a day, and on seeing the dark volume of smoke pouring from the chimney of a Wood street small manufactory, over into the storehouse of my friend Campbell, of Campbell & Dick, I wondered how they got along in a "*dry-goods*" store, with the smoke pouring into their windows. In cases of that kind smoke should be abated.

Mr. President, ladies and gentlemen, we have heard from a resident of, and a visitor to, Birmingham, England, what the facts are there relative to smoke abatement. We have heard something by letter from an agent of the Smoke Prevention Society or Committee, supplemented by Professor Phillips, of what is being done in Chicago. We have here with us to-night a member of that prevention society, who has been long identified with the interests of Chicago, and is thoroughly familiar with all the movements of public interest therein. Colonel Wm. P. Rend, whom I take pleasure in introducing, will state the case as it is in Chicago, and perhaps from his knowledge we may derive some benefit.

COL. W. P. REND: I came here as a listener, not with any intention of speaking. I am not prepared to speak, but the subject is so full in matter for thought, I feel that I should avail myself of this courteous invitation to express a few opinions upon this burning subject of the day.

A great deal is said by the opponents of smoke that is said with honesty, but that is most fallacious. It is one of the most difficult questions in the range of chemistry. It has been discussed in England for over six centuries. There have been organizations such as you have here to-day, and such as we have in Chicago, of which I am a member. In the Middle Ages, King Edward II. issued a royal decree against smoke consuming, but afterwards he appointed a smoke commission, or a commission to destroy the furnaces from which smoke was produced. I am afraid there are a great many in Chicago, and some in the city of Pittsburg, who are equally radical to-day.

The editors, two or three centuries ago in London, thundered against this evil. As I lately said in Chicago, I believe there must be something in that new-fangled religion, which I believe they call Theosophy, which teaches the reincarnation of disembodied spirits. I believe some of these editors who are writing on the smoke question are the self-same editors who wrote in London three centuries ago.

It is said here that smoke is an evil, it is a nuisance, it is a crime, it is destroying life! One of the learned members of the profession, although doctors disagree, said that it is killing men. Now, I am not a doctor, but if I was, I probably would differ with that gentleman; but I express the humble opinion of a layman that this is wholly fallacious. I believe that smoke is healthy. I challenge any doctor to prove that it is unhealthy. Why, the doctors, thirty years ago, in treating the cholera here, not only used in one form the smoke that was coming from your chimneys, but they ordered fires built in the streets of Pittsburg. Was not that for the purpose of destroying the germs, the bacteria, and all bad matter, thus killing the germs of disease? I have frequently thought that your great exemption from grippe this winter was due to your smoky chimneys.

Now, there are two views to take of this—there are several, in fact: first, as to whether it is healthy; second, as to whether it is destroying vegetation. These are the two main aspects of this case. First, the view that it is unhealthy. The doctor says that carbon helps to destroy noxious exhalations, the putrifying, decaying matter. Now, the doctor can go still further and tell you it is an antiseptic. It is used in the shape of soot or carbon, the same as charcoal, I believe, in purifying water. Of course, you take a little in the lungs, and you know the air passing through your lungs will be purified by a little carbon. During the cholera times the doctors considered it a specific for cholera. It is laid down as an excellent agent in medicine.

Second, what is it that kills your vegetation? Is it the carbon or soot? Gentlemen who spoke here very ably said, in the worst of cases, soot did not amount to over 3 to 5 per cent. of the component parts of what you call smoke. Now, then, according to this statement, the soot is not injurious. What kills your vegetation? Is it not the fumes of sulphur, the acids? Suppose you eliminate the smoke, the gases still remain.

Let us turn now to another thing. We are told that Pittsburg should be a place for men who can enjoy their wealth, the millionaires. But, gentlemen, there are other men besides retired millionaires. You have thousands and tens of thousands working in these mills, who are drawing their support from these manufactories that cause this smoke. Your property, instead of being injured, has been enhanced in value many fold by reason of the manufactories that make this smoke. The very soot that falls upon your ground increases tenfold the value of your property. You can do away with smoke, you can do away with the burning of coal, you can convert your city into a country village or into a country graveyard, but where these manufactories are is the very life of your city. It is the support of the doctors, the lawyers, your engineers, your other men who are here. The soot is the very life of your city.

Now, you say smoke can be prevented. It can be to a certain extent. I am with you in a reasonable way, in every practicable way, by the devices of scientific research or mechanical invention, to reduce this to the minimum, but after you have done all this, you will have soot. Of course, you deplore this; but natural gas is becoming practically exhausted for manufacturing purposes, and you are averse to going back to coal.

I certainly do not blame any one who tries to use every reasonable effort to diminish the volume of soot and smoke. It is right; it is proper; but you are asking for prohibition here. In Chicago they are asking the same thing. Let me ask these ladies, can you build your fire in the grate without having a large volume of black smoke when that fire is built? Ever since fire was brought down by that fabled personage, Prometheus, you have never been able to build fire with coal without smoke. If you feed that fire regularly, put on a regular supply of fuel at any one time, it will be a little less, perhaps; the amount of smoke, say, from the chimney, will be smaller at any given time, but in the aggregate there will be a great deal of smoke.

Now, then, here is the difficulty, devoid of all technical terms. When you put coal into a furnace, it undergoes two operations—first, the disintegration of the coal into its elements, which takes place at a low temperature; then combustion follows at a high temperature. You cannot, when you start a fire, get this high temperature, nor produce complete combustion, nor any approximation to it. If you transfer that fire to the furnace of a locomotive or a stationary engine, you observe identically the same things as appear in a common grate.

It seems to me that there are a great many suggestions here to-night (I say it with all due respect) which it would be most pernicious to adopt. I take it that the manufacturer who invests his money, who creates values, who gives employment to vast numbers of men, should not be treated as a common criminal. If you do that, you will find that course full of danger. The manufacturers in Chicago have been convicted, they have been hounded, and they have been prosecuted by the minions of the law, by the man whose letter was read here to-night. They are dragged before the magistrates and fined $50 for something they cannot prevent. They say: "We will co-operate with the Society, we are willing to do everything in our power, we will do all that we can do, but after all we do object to being treated as common malefactors, after building up the city."

Letters have been received by the manager of one of our largest manufactories in Chicago from nearly all the leading manufacturers, in which they express a willingness to form a society for the purpose of resisting this persecution and this tyranny. Now the manufacturers, as Prof. Phillips says, are willing to co-operate in any movement that is practical, in any movement that is just, but they do object, and the manufacturers here will object, the men will object, if a course of persecution be pursued towards them.

I thank you for listening with so much patience to remarks that perhaps are not in harmony here with the sentiment of the meeting, but this question, which has been so engrossing the attention of the people of England, of the inventors and scientific men of England, for so many years, so many centuries, you must not expect to solve in a single day. Reduce smoke as far as possible, but with due regard to the interest of the manufacturer.

I understand a very large proportion of the combustible elements of coal used on locomotives escapes through the smoke-stack. If you will point out to these railroad companies how they will consume that, it is likely they will look upon you as a benefactor.

MR. METCALF: Mr. Chairman, I think there is very little, indeed, to be said in conclusion. There were one or two remarks made I would like to correct. One by Professor Phillips, in regard to the sulphur as between anthracite and bituminous coal. If he had listened to the paper he would not have heard any allusion to sulphur. I said the fumes given off by anthracite were quite as deadly as from bituminous.

I think my friend Mr. Koch, will agree with me that steel cannot be heated safely in an entirely smokeless flame, even in a Siemens furnace. I agree with his remarks in general, and especially in regard to iron; there, the temperatures are so high that great care must be used to prevent burning; therefore the iron manufacturer must have even more latitude in smoking than the steel manufacturer.

President Hunt: The time of the meeting has gone by. I know there are some who would like to talk on the subject, but I think it will probably be better to defer that until another evening. I will say, in closing, that after all that has been said, the facts are that if we can abate any portion of the smoke of Pittsburg I think there is no one of us but will agree that it will be for the best interests of the city. No doubt about it, as Mr. Metcalf has said, some of the smoke is necessary; but I again call attention to the other fact which he said as plainly, that a great deal of the smoke that does come in the community is unnecessary and can be abated; and it would be to the best interest of the city if it were abated.

But what we want brought out is something practical as to what we can have to abate the smoke; what methods, what apparatus can be recommended to the manufacturers for abating the smoke. That is a subject which the Engineers' Society will be be very glad to have papers on in the future.

Adjourned.

R. N. Clark,
Secretary.

Meeting of the Chemical Section of the Engineers' Society of Western Pennsylvania.

March 11th, 1892.

Preliminary meeting called by John W. Langley and James O. Handy (chemists and members of the Engineers' Society).

Thirty-four chemists were present. President Alfred E. Hunt, of the Engineers' Society, explained the privileges which the chemists would have as members of the Engineers' Society, and suggested that they choose a temporary chairman and secretary. Prof. John W. Langley was nominated by Mr. Handy for chairman, and Mr. James M. Camp by Prof. F. C. Phillips for secretary. Both were elected.

Prof. Langley spoke in favor of joining the Engineers' Society, saying that the chemists would thereby ally themselves with the strongest local society and have at once the privileges of knowing its members and of using its library and rooms.

Messrs. Jesse Jones and C. P. Van Gundy favored an alliance with the American Chemical Society, saying that it had a membership of 400 and was forming a strong national organization.

After some discussion, and on motion of Mr. Handy, a committee was appointed to consult with the board of directors of the Engineers' Society as to the possibility of the formation of a chemical section of their society. Messrs. Phillips, Hopké and Handy were chosen.

On motion of Mr. Van Gundy a committee was appointed to consult with the American Chemical Society as to the advantages of becoming a section of their organization. The committee consisted of Messrs. Van Gundy, Harvey and Carnahan.

Mr. Handy then read preliminary notes on a rapid volumetric method for phosphorus determination in iron and steel. His results indicated that the method would prove rapid and accurate in use. He promised to give the results of further work on the method. The paper was discussed by Messrs. Hopké, Carnahan and others.

Mr. Jones introduced the subject of the "Freezing of Sulphuric Acid," and asked if any one knew the cause. Mr. Stahl and Mr. Eastwick explained that there were three critical stages in the dilution of sulphuric acid at which it would freeze. Acid was generally sent out all right by the manufacturers, but careless consumers might leave the carboys open and the moisture taken up from the air would dilute the acid enough to permit it to freeze in cold weather. Prof. Langley had had this experience.

As to organization, Mr. Handy thought that much good could be accomplished by the Chemical Section:

First. In following the progress of the science of chemistry as shown in the various magazines. Committees could be detailed for this work.

Second. Investigating new methods.

Third. Working out of new methods.

He said: "Although few of us think that we have time for much more work than we are doing, I venture to say that we are in many cases deceiving ourselves. A man *can* do what he really wants to do."

The subject of "Sulphur in Soot" was brought up by Mr. Hopké. It seemed to be the opinion of those who had investigated it, that the sulphur exists as a gas (probably sulphurous oxide) held in the pores of the carbon.

After appointing March 24th as the date for the next meeting the chemists adjourned.

MARCH 15TH, 1892.

THE regular monthly meeting of the Society was held on March 15, 1892. Mr. Alfred E. Hunt, President, in the chair. One hundred and four members and visitors were present. Among the latter were representatives of the Ladies' Health Protective Association of Allegheny County. The minutes of the last meeting were read and approved. The following applicants were elected to membership: Albert E. Duckham, Daniel McGary, W. T. Manning, Neville C. Davison, Frank E. House, P. Brynn, J. F. Kunz, O. Sivertsen, G. W. Schluederberg, Selwyn Taylor, P. C. Patterson, Chas. I. Pierie, B. E. V. Luty, F. H. Mollenauer, John H. Nicholson, O. C. Opsion.

The following communication from the Board was read by the President, in regard to the forming of a Chemical Section:

MINUTE OF ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA—BOARD MEETING MARCH 15, 1892.

By invitation the Board received the Committee of Conference appointed by the Chemists—F. C. Phillips, J. O. Handy, T. M. Hopke—who made the following report:

At a meeting of Chemists held March 11th, in the rooms of the Engineers' Society, the undersigned were appointed a committee to consult with the Board of Directors of the Engineers' Society with reference to the formation of a Chemical Section within the Engineers' Society. Accordingly we would respectfully request you to determine whether an organization of the following kind would meet with the approval of the Board. The proposed Section would include only members of the Engineers' Society in full standing. Meetings would be held in the rooms of the Society, at regular intervals, for the discussion of subjects of a chemical nature; the affairs of the Section would be managed by officers to be chosen by its members. The general purpose of the Section would be the promotion of pure and applied chemical science. Respectfully,

FRANCIS C. PHILLIPS,
JAMES O. HANDY,
T. M. HOPKE.

MINUTE OF BOARD MEETING, MARCH 15, 1892, ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

Resolved: That the communication of the Chemists be submitted to the Society for its approval, with the *proviso* that the affairs of the Section referred to in the Committee report, as being managed by officers to be chosen by the Chemists, be limited to matters purely technical and professional, and they shall not bind the Society in questions of policy or expenditure.

R. N. CLARK,
Secretary.

On motion of Mr. J. W. Langley the resolution of the Board was adopted. The president then called attention to the circular addressed to all members in regard to obtaining the necessary funds for defraying our quota of $650, towards the expenses of holding an International Engineers' Congress in Chicago during the World's Columbian Exposition of 1893. The president explained that all subscriptions from members are now due, that all the societies contributing are asked to make immediate payment to the fund.

The Secretary announced the death of our late member, B. L. Wood, Jr. Mr. A. Dempster was requested to present the memorial.

The Secretary called attention to the receipt of a letter from O. Chanute, Esq., dated Chicago, February 25, in regard to the Fifth International Congress of Internal Navigation, enclosing the report of the Committee on Promotion.

The question for the evening was the

DISCUSSION OF MR. METCALF'S PAPER ON SMOKE.

PRESIDENT HUNT: The meeting this evening, according to the call of March 12th, is for the discussion of the paper of Mr. William Metcalf on Smoke, which was read at the February meeting. The Board has instructed me to announce that the discussion will be limited to the points mentioned in the paper of Mr. Metcalf; that is to say, the action of flame and the control of smoke produced when burning soft coal, and the description of smoke consuming appliances, with remarks on the practical management and theoretical value of the same.

In addition to this the Board has allowed a little broader interpretation than was put in the notice to the effect that there are several friends of the Society who feel that the subject, not only in the technical view mentioned in the paper and in the notice of the meeting, but also in the broader view of the case as regards the effect of smoke in our city, they should have the opportunity to discuss the matter. Therefore, I will call upon several to-night to take part in this discussion, whose direct line will not be technical in an engineering point of view. (The Society is not responsible for the statements contained in any of these communications.) I will call upon Prof. Langley to open the discussion.

MR. J. W. LANGLEY: This paper calls attention to one side of the smoke question which is sometimes overlooked, viz., that certain industries necessarily produce smoke because of the requirements of the material which is being heated.

Extremists are too apt to consider the matter only from the point of view of the customary failure to obtain complete combustion. Now, chemistry and physics show that the smokeless burning of bituminous coal, or, indeed, of any hydrocarbon fuel, is only possible by having an excess of unconsumed oxygen in the upper part of the flame, and this free oxygen is prejudicial to the manufacture of wrought-iron and of steel in certain stages of the work.

But while this is true, as Mr. Metcalf states, it would appear that we need to produce only a little smoke, just enough to show that the oxygen is largely consumed and the carbon is slightly in excess. The last four years' experience of this city with natural gas has demonstrated this, for even the puddling-furnaces have been run with comparatively smokeless fires, and no appreciably greater loss of iron or impairment of its quality has ensued; so it would appear that even in this typical smoke-producing industry the theoretical, or what may be called the chemically necessary, production of smoke may be very much reduced over what is now tolerated when these furnaces are fed with coal. Steel furnaces burning either natural or producer gas do not, under what is now considered good practice, produce any very large quantity of smoke.

Setting aside puddling-furnaces, coke-ovens, and household fires, the largest single smoke-producing agency is undoubtedly boiler fires. Probably in Pittsburg they equal all other causes of smoke combined, and it is to them that the coming inventor needs to direct his attention. To-day the mechanical stoker nearly solves the problem where it is used, but small establishments cannot well afford to pay for putting them in, and it is from these collectively that the greater part of the smoke comes.

I saw in Detroit, some years ago, an invention applicable to single boilers which suppressed the smoke completely. The fire was divided longitudinally by a double iron partition, having through it laterally numerous channels of communication in the shape of short tubes three inches in diameter. The whole was kept cool by water. The fire was thus divided into two, and by a reversing damper the draft could be sent at will in either direction through the partition and then out to the stock. When fresh coal was charged into one fire, its smoke was driven across the other one, which was then in the condition of glowing cinders; thus the smoke was always ignited and burned. The device was somewhat expensive, but it raised steam very fast.

The cost of this appliance would seem to be the only thing against its adoption; but this fact would also seem to show that the suppression of smoke from boiler fires is only a question of spending a little money.

Finally, there is one evil effect of a smoke-laden atmosphere which can hardly be too strongly emphasized, and that is its mental influence. There does not seem to be any strong testimony that smoke is directly injurious to bodily health, but no one will deny that it exerts a most depressing effect on the mind and spirits.

Men need sunlight almost as much as plants do, only in the former case the deprivation shows its effect rather on the general temperament than on muscular well-being, cutting down those impulses to cheerfulness and to an appreciation of the amenities as well as the ideal side of life, which are all too feeble anywhere, but which are almost paralyzed by months of a heavy winter smoke-burdened atmosphere.

DR. W. A. DALY: I was invited by the ladies of the Health Association of Allegheny County to speak to you, and it is in response to that request that I appear before you this evening.

That the parallel between the smoke-gauge and the manufacturing prosperity of our city was most interestingly discussed by Mr. William Metcalf at your last meeting, there can be no question; but there were some views expressed that all cannot coincide with.

Let us examine for a moment his assertion, "That there is nothing particularly unhealthy about smoke." Of course, the deleterious effects of smoke upon the health of any individual or the community must be *pari-passu* with the quantity of smoke inhaled.

Its evil effects will also be largely determined by the ability of the individual to expectorate or reject the carbonaceous deposit formed within the air-passages. But it must be borne in mind as a cardinal basis of fact, that the human economy was not designed by the Almighty as a smoke-consumer. But it is, alas! also true that poor humanity can stand terrible abuses of its constitution in this as well as in other ways.

The atmosphere of Pittsburg, in the years when smoke was all prevalent, was justly feared by strangers unused to its irritating effects upon their throats and respiratory organs, and the "Pittsburg Grippe" was by no means a mythical affection to those who came to Pittsburg to use their voices for our entertainment.

Its evils upon the air-passages of the weak are much more seririous, proportionately, than upon the strong; as the former have not the repellant vigor to get rid of the carbonaceous deposit upon the mucous membrane of the lungs, and a large proportion of these minute, sharp particles work their way into the lung tissues. Every medical man, who has made post-mortem examinations upon deceased Pittsburgers, has recognized the effect, portrayed in the darkly-mottled lung-tissue, filled with foreign carbonaceous matter.

Well, you may ask, does it hurt to have these mottled lungs filled, as they are, with the coloring-matter of smoke and soot? You might also ask, if it hurts to spur the jaded horse, and whether or not the horse is the better or worse for it after he reaches the stable, after a long and weary journey?

It is true, people have been known to go through a long life with only one lung, the other hopelessly damaged, and the working lung nothing to brag of. The wearing of one lung will scarcely become fashionable from election, yet the mottled lung is the fashion in Pittsburg to-day. Through the blessings of a gas-period, the mottled lung is somewhat past date, and may have faded some of its richly colored meerschaum-pipe hue, yet the fashion is rapidly coming in again, with the second advent of smoke.

The deposit of foreign substances in the lung-tissue, whether it be the residuum from smoke, soot, the dust from the stone-cutter's chisel, or that from grinding tools, or of the fine particles that escape from wheat in the manufacture of flour, the dust of the grain in the malt-house, all induce consumption of an incurable character. Since the number of citizens who are exposed to the inhalation of smoke and soot, in Pittsburg, are in the greatest majority, so of equal pace are cases of consumption, once established from this cause, more difficult or impossible to cure.

Mr. Metcalf stated that, " A reference to the statistics will show that Pittsburg is not so particularly unhealthy, but on the reverse it enjoys a rather low death-rate."

It ought to be borne in mind, that Pittsburg is situated in a most advantageous location, and I might be pardoned if I would remark that her physicians are skilful in combating the evils caused by smoke, soot, and other insanitary conditions that are tolerated in excess in Pittsburg. Yet, notwithstanding these advantages, we have a death-rate for 1890, larger than Cleveland, St. Louis, Chicago, Buffalo, Detroit, Milwaukee, Louisville, Indianapolis, and other cities, none of which are as eligibly situated for perfect hygienic and sanitary protection as Pittsburg.

And here it might be well to state, that in comparing the death-rate in Pittsburg, in the eight years of natural gas, with the eight years previous when coal was universally used, there is a diminution of the death-rate in the gas-period of 1.62 in the 1000, and this diminution means a very considerable sum-total in favor of the comparative non-smoke period, as we in the last two years had more than our share of fatality from the pandemic of " La grippe."

Now, if we take the period of 1884 to 1889, the five years when gas was cheap, and used in nearly every house and mill, we have a death-rate of only 19.58 per 1000. In the years 1890 and 1891, a period when gas was high and less generally used, our death-rate was raised to 22.15 per 1000, a difference in favor of the cheap-gas period of 2.57, so that, which ever way we turn, there is a positive improvement in the lives and health of the people, even in the face of a fatal pandemic of " La grippe," and the terrible abuse of natural gas by superheating houses.

A gentleman of London was once conducting me to his works. As we approached, I asked him if the works were stopped; he said "No," and asked why I asked. I answered, I saw no smoke rising from the stack; he answered, with a smile, "If we emit smoke the authorities inquire into the matter in a rather disagreeable way, which we don't fancy, and if a stoker allows his stack to emit much smoke, we have no use for him, for he would get us in trouble."

In former years, when smoke was more prevalent than now, I had patients ill with throat and lung affections, that could not live in Pittsburg, though Pittsburg born and reared. Shall we get that undesirable condition of atmosphere back again? I hope not. Some evils die hard, but they must die, and the emission of dense smoke from every stack and chimney is an evil that can be, and is largely remedied elsewhere, not only in domestic life but in the arts and manufactures; this is no longer doubted.

Mr. Townsend: A brief description of the construction and operation of the Roney Mechanical Stoker may be of interest to those who have never had an opportunity to see one in operation.

The fuel to be burned is discharged into a hopper on the boiler front. In large plants it is usually handled, direct from the car to the hopper, by elevators and conveyors. Set in the lower part of the hopper is a pusher, to which is attached, by a flexible connection, the "feed-plate" forming the bottom of the hopper. The "pusher," by a vibratory motion carrying with it the "feed-plate," gradually forces the fuel over the "dead-plate" and on the grate. The grate consists of horizontal, flat-surfaced bars, reaching from side to side of the furnace, carried on inclined side bearers, extending from the throat of the hopper to the rear and bottom of the ash-pit. The grate bars, therefore, in their normal condition, form a series of steps, to the top step of which coal is fed from the "dead-plate." Each bar rests in a concave seat in the bearer, and is capable of a rocking motion through an adjustable angle. All the grate-bars are coupled together by a rocker bar, a variable back-and-forth motion being given to the rocker bar through a connecting rod and crank on shaft.

This variable shutter-like movement furnishes a simple means of regulating the quantity of air passing through the grate, according to the amount of coal to be burned to supply the demand for steam.

Assuming the grate to be covered by a bed of coal, and fresh fuel being fed in at the top, it is obvious that when the grate-bars rock forward the fire will tend to work down in a body. But before the coal can move too far the bars rock back to the stepped position, checking the downward motion, breaking up the coke thoroughly over the whole surface, and admitting a free volume of air through the fire.

This alternate starting and checking motion being continuous, keeps the fire constantly stirred and broken up from underneath, and finally lands the cinder and ash on the "dumping-grate" below. By releasing the dumping-rod, the dumping-grate tilts forward, throwing the cinder into the ash pit, after which it is again closed ready for further operation. The "dumping-grate" is made in two or more parts, so that each can be dumped separately. The operation of the stoker, therefore, consists of a slow but continuous feed, a constant stirring of the fire, and an automatic rejection of the cinders, all performed without the opening of fire doors.

For the purpose of facilitating the smokeless combustion of the coal, provision is made for supplying heated air to the furnace. Air is admitted through holes in the stoker front to a hot-air chamber over the throat of the hopper and in front of the arch; it there passes through perforated tiles to mingle with the gases from the fresh fuel at the top of the furnace.

A coking arch of fire-brick is sprung across the front of the furnace, covering a part of all the grate, according to the kind of boiler to which it is applied. This arch forms a reverberatory furnace, whose action is to thoroughly coke the fresh fuel and release its gases before ignition.

Not the least important element in the smokeless combustion is the steady supply of the coal in a small but continuous quantity onto the coldest portion of the grate, where, after coking, it is steadily carried down to the hottest portion of the fire at the bottom and finally consumed.

The actuating mechanism of the stoker is simple. All motion is taken from one driving shaft. This shaft is driven from a small engine attached to the stoker front and consuming a hardly measureable fraction of horse-power. The driving shaft carries a disc and wrist-pin or a crank, from which a link couples to the "agitator." Through the eye of the "agitator" passes a stud screwed into the "pusher," on which stud is a feed-wheel, by which the stroke of the pusher, and consequently the amount of feed, is regulated.

In like manner the rock of the grate-bars can be adjusted between any limiting angles, and over a range of motion from no movement to full throw, by means of a "sheath-nut" and "lock-nuts" on the "connecting-rod," and the amount of stirring which the fire receives regulated according to the demand for steam. By these two simple adjustments, within the comprehension of the ordinary fireman, the whole action of the stoker is controlled, and the fires forced, checked or banked at will. There are small doors in the front on each side of the hopper, giving access to the furnace above the grate when necessary.

MR. JAHN: The McKenzie Economic Smokeless Furnace. This furnace is erected in four chambers so arranged that during the process of combustion the heat is not allowed to touch the boiler or any cooling surface. This is the main principle. It is built vertically and not longitudinally, as was the old style. This, among other advantages, results in the saving of space. The old furnaces were built out from the boiler, while the McKenzie is built under it. The coal is thrown into the front of the furnace, the end of an arch marking how far in the fireman is to throw it. There it becomes coked, and the gases pass into an arch, the space beyond which is left full of the coal already coked. Incandescence is produced in the baffle and bridge-wall after the third fire, air being introduced through the bridge-wall. The heat, which is raised to a very high temperature, 2200 to 2500 degrees, is retained. The higher the temperature the more oxygen it takes up. Oxygen, combining with the carbon and hydrogen, produces carbonic acid gas, which is, of course, invisible. This is the simple secret of the McKenzie, which is a positively smokeless furnace.

There are now four of these furnaces in operation in this city, viz., two at Weyman Bros., Duquesne Way; one at Hugus & Hacke's, Fifth Avenue, and one at Gensenleiter's livery stable, Eighth Street.

Mr. Cyrus Smith: To follow Mr. Metcalf's paper as you request, I shall have to make my remarks brief. With regard to flame, the more flame we have under the boiler the more steam is generated, and the more smoke. Temperature is lowered by fresh air being admitted, and it is lowered by the fuel that is thrown in. Now, to prevent the lowering of that temperature we should apply a hot blast. And if we want economy of fuel, we want to have the ash-pit closed up to prevent the cold air from entering. We want the fresh air entering to be warm; in other words, to apply a hot blast to warm the fresh air entering, thus preventing smoke to a large extent. My idea is to admit air at a temperature that may be 300, 400 or 500 degrees instead of 60, 90 or 100 degrees, as the case may be. The saving in fuel by that process would probably be 35 per cent.

Mr. P. Barnes: I can simply express my convictions in a very few words on the general topic. One is, if anything has been shown clearly in this world, these two things have been shown: first, that the economical flame is a smoky flame, taking the word "economical" in its general sense. Then the really efficient fireman, call him what you please, is the smoke-preventer.

Mr. Max Becker: I was, unfortunately, prevented from being present at the meeting when the paper was read by Mr. Metcalf, so I do not know all the points that were brought forth in the original paper, although, from the discussion, I can readily see what the substance of the argument amounts to. Now, to a man who spends as large a part of his life on railroad trains as I do, the abatement of the smoke nuisance is just as important as to the peaceful citizen that dwells within the grimy walls of Pittsburg. And that there can be an abatement of the smoke on railroad locomotives I think is no longer doubted.

I know that the railroads have in general use certain rules for the guidance of locomotive firemen, and the quantity of smoke emitted is very nearly proportional to the rigidity with which the firemen obey these rules. I recollect that upon a trip on a Pan Handle train the absence of smoke attracted my attention, and after we reached the relay station I thought I would go forward and congratulate the fireman upon the careful attention that he had given to the stoking. But when I reached the locomotive I found that the master mechanic of the division was stepping off the foot-board, and he informed me that he had been doing the firing himself that trip.

I think if firemen would observe the lessons, like the one that was given by the master mechanic, there would be a great deal less of the smoke nuisance on the railways of the country. Of course, on a locomotive, it is purely and simply a question of fuel consumption. The points which were brought forth in Professor Langley's remarks as to the effect of heat on the product of manufacturing do not affect a locomotive, because it is all consumption and no product. Therefore, I think it would be much easier to effect an abatement on locomotives than in manufacturing establishments.

Mr. A. Dempster: Do you mean abatement or abolition?

Mr. M. J. Becker: I mean abatement.

Mr. C. Hyde: It seems to me that, to a certain extent, we have rather confused the two conditions under which smoke is produced in the city—the manufacture of iron and steel with steam production. Now, of course, it is obvious to every engineer connected with iron and steel manufacturing that there are processes where smoke must, of necessity, be made—in the puddling process, in reheating iron or steel, and a number of cases of that kind. But granting all that, there is an enormous amount of smoke made which is not necessary. We may just as well admit at once, that while some is necessary, much is unnecessary, and we should look round and see if we cannot find some way to get rid of part of it.

The business blocks and small manufacturing concerns, which have a boiler in the basement, are the principal offenders, as far as the city proper is concerned, and these establishments frequently do more to pollute the atmosphere than do the iron works where the production of some smoke is admittedly necessary.

Experiments have proved beyond a doubt that a great deal of the smoke ordinarily produced under a boiler can be prevented. Mechanical stokers have considerably simplified the question of smoke prevention, and, it seems to me, have shown an economy in fuel as well. Not, perhaps, as large an economy—my experience does not show as large an economy at any rate as is claimed by some of the manufacturers of mechanical stokers—but undoubtedly there is an economy in fuel consumption by their use. This economy is not measured entirely by the amount of black smoke consumed, which probably represents from 3 to 5 per cent. of the fuel, but, in addition, some of the carbonic oxide, which would escape unburned under other conditions, is completely burned. Great benefit is derived, too, from the fact that the rush of cold air through the furnace every time the firing door is opened is avoided by the use of some mechanical appliance. Taking it altogether, some 10 to 15 per cent. of the fuel can frequently be saved by the use of a well-designed mechanical stoker, and a considerable saving can often be effected simply by having the furnace properly constructed even where no mechanical arrangement is used.

In small plants the initial cost of mechanical stokers make the use of them somewhat prohibitory, but, by a cheaply constructed furnace where the lighter hydro-carbon gases, which rise directly the fresh fuel is thrown onto the grate, are deflected by an arch onto a bed of incandescent fuel at the back of the grate, the smoke is practically all consumed, and of such a furnace I have made a personal test.

The bed of incandescent fuel at the back of the grate in this furnace was comparatively thin, and allowed more air to pass through than was needed for its own combustion, which air being heated up to a sufficiently high temperature combined with, and completely burned the smoke from the green fire near the door where the supply of air was short. The test did not show a very large economy over the ordinary grate, about 5 or 6 per cent., but the smoke was almost entirely consumed. As smoke prevention is the main point we have in view to-night, that is a matter of some moment to the people in the business part of the city, and, in my opinion an arrangement of that kind—I won't mention the name of the furnace here, as I am not advocating the use of any particular type of furnace or stoker—can be put under a boiler of any type, and will do away with nearly all the smoke.

Mr. W. Metcalf stated in his paper that mechanical stokers, whilst doing away to a certain extent with smoke, reduced the capacity of the steam plant, necessitating the use of more boilers. Now I have made two tests, in fact several tests, of boilers fitted with mechanical stokers in order, mainly, to determine how the capacity of those boilers was affected. A test was made about twelve months ago with a view to being able to advise the Ætna Iron & Steel Co., Bridgeport, O., for whom I was consulting engineer, more intelligently as to the stoker to be used under the boilers for their new plant. The test of the boilers showed an average capacity of over 50 per cent. above their normal rating, whilst the economy remained decidedly high. Mechanical stokers were put in, and two weeks ago another test was made of the new plant to see whether that showing was borne out, when a capacity of 88 per cent. above its rating was obtained. It seems to me that, if a sufficient grate surface is allowed, the question of capacity need not enter into the case where a mechanical stoker is used, any more than where an ordinary furnace is used.

The labor saved by the use of mechanical stokers is a matter of considerable importance, too, especially in large plants. In the plant referred to there are three boilers, of nominally 450 horse-power, though they are regularly developing 750 horse-power, whilst one man easily attends to the three. The objection to stokers on account of the difficulty in the removal of clinkers seems to be fairly well met by the experience with this plant where the amount of actual clinker removed averages 17 per cent., and is removed, without difficulty, in regular work.

Of course this saving in labor applies more to large plants, but even in the business part of the city where we have a number of small steam plants of from 100 to 200 horse-power, a stoker could easily be applied and show, I am persuaded not only economy in fuel, but would certainly lead to a very great improvement in the atmosphere and general appearance of the city, to the benefit of the community as regards health, comfort and cleanliness.

Mr. D. Ashworth: I have had considerable to do with furnaces, especially in the generation of steam, and it does seem to me that the question of the abatement of smoke, especially in the generating of steam under steam boilers will resolve itself into this fact—that it means larger capacity of boilers to produce the given work. It means slower combustion. You will find that where a great volume of heavy smoke is emitted from our manufacturing establishments it is where the fires are violently urged, where the capacity is below what it should be.

I have personally made tests and been brought into contact with various stokers, among others with stokers that have been brought over from the other side, and with a slow combustion and with capacity to generate the steam necessary to do the work, we have the smoke reduced to the minimum, but when you drive them you get back to the original condition all the time. I have visited plants where these devices have been said to be in operation successfully. I have had notices brought to me and presented to me of the abatement of 90 per cent. of the smoke, and these testimonials have been of the most glowing character.

I was requested to visit a plant not long since equipped with this device. I inspected the plant, and when I left that establishment a mile away from it, from where the boilers were located, I could see vast columns of dense smoke ascending heavenward, excelling anything seen in the days of old Pittsburg smoke. Nine-tenths of the testimonials emanate from establishments and plants where they have a superabundance of steam capacity, where they have slow combustion, where the work is performed in a leisurely manner. That can not be denied, and I challenge anybody to deny it. I have watched it closely and the violent urging or the overtaxing will produce the result of making heavy smoke. My experience goes back to the period in Cincinnati when they passed the smoke ordinance, and every effort was being made, just as we are interested to-day, in abating the smoke nuisance. I have sat upon the wharf on the Kentucky side and looked at a stack for two hours equipped with a mechanical stoker, and the moment those fires were urged by the fireman, I noticed that we got back to the original condition.

Now going right back to this nuisance here in the city, to-day as I looked from my office window I could see vast columns of smoke emitted from chimneys that project a few feet above the building proper. Now such things as that can be abated. We all desire the abatement of that but we allow ourselves to be carried away by every gimcrack notion that comes along and professes to be a smoke abatement, smoke preventive or smoke abolisher, and yet not one of them is a success or make the fire smokeless when it is urged.

But coming down to our manufacturing, our iron and steel industries, where drive to our utmost, where there come times when steam must respond quickly, the generating part must respond quickly, no stoker will fully come up to the requirements. The whole history of mechanical stokers bears this fact out that they do not respond quickly. If you have a superabundance of boiler pressure, if you have large batteries with a superabundance of steam power, the stoker will work with great accuracy as regards the minimum of smoke production, but where it comes to plants which must respond so quickly as we do in iron and steel making, with the high pressure of steam, it destroys the very power that so far has been brought forward in the stoker. I am an earnest advocate of stokers. I am an earnest advocate of anything that will abate this nuisance, but I do say right here that we have been flooded, as other cities have been, with testimonials of smoke saving devices that are not worth the paper they are printed upon. That is the idea.

MR. J. M. BAILEY: I cannot add very much to what has already been said. I fully agree with the last gentleman that spoke. I have been investigating mechanical stokers somewhat but in my opinion I have never yet found one that is applicable to rolling mill practice. In other words as a rolling mill is constructed at present, the fires under the steam boilers must respond, as Mr. Ashworth very well says, at times to violent urging and calls upon them at such times the fireman usually fails. My idea of abating this smoke nuisance, is that we must start a school for educating the firemen. We make a mistake in placing the most ignorant Polander or Italian in charge of such work. We ought to have some one who understands, and with whom we can talk and reason. That is not the class of men we have at present. My experience is that all good firemen have either been promoted to something better or else have died out. I frequently spend some time in trying to educate some people in the proper way, but they invariably go back to their old practices when my back is turned.

I reside on the hill just above some of these stacks and I would like very much indeed to have this nuisance abated. I have made some changes in our works, by introducing steam right over the fire and I have succeeded in abolishing this smoke nuisance at our place almost entirely, except just at the time when they fire with green coal. I can not keep them from throwing the coal back to the back end of the grate-bars, but by the time they get over the battery of boilers (where there are four or five boilers in the battery), and the fireman ceases firing and shuts the doors, the smoke is almost entirely gone. This is accomplished by putting the steam over the fire by blowers spaced about two or three feet apart, and by boring a hole some four inches in diameter right through the fire-front and taking in nothing except the air.

But if I could, as I say, going back to the original point, only get the fireman to do what I tell him I think I could abate the smoke nuisance at least 90 per cent. I would just like to call the attention of the persons interested in getting the smoke nuisance abolished here in the city that they seem to be going for the parties who own the establishments. Now, with all due deference to them and their judgment it seems to me they ought to proceed against the people who make the smoke. If any one of us has an establishment and we hire a man who professes to be a fireman and we put our boiler fires under his charge and we pay him for his services and he does not obey our instructions, he ought to be fined and not the person who hires him. That is my opinion. I may be entirely wrong but there is where I would like to have the police employed, and the fines placed.

Mr. C. B. Albree: I have been re-setting my boilers lately with the object of finding how much it cost to run my furnace. We made a little change proposed by my foreman in the manner of burning the coal and it seemed to do away with the smoke almost altogether. Formerly we burned it in the ordinary way. Now in rebuilding we made the bridge-wall of two walls of brick leaving a space so that the air could go up through the middle of the bridge-wall striking the flames as they come from the coal directly underneath the boiler, admitting simply cold air. We found this simple arrangement did away with the smoke almost entirely except when new coal is put in. So far as the saving in fuel goes the difference did not amount to much, but we did find that we had a finer quality of ash than we had before.

MR. T. P. ROBERTS: I have had perhaps the ill-fortune to live in about twelve different parts of Pittsburg, up on the hills, over in Allegheny, in the East End and in various portions of the city, at different levels from the water-line up, and during the period of twenty-five years I have watched with a great deal of interest the direction of smoke clouds in old times, and I am very sorry to see the growing intensity now since the failure of natural gas. There is one point which does not seem to have been touched upon practically. What we are concerned over in Pittsburg is the damage done by smoke to our household furniture, works of art, etc. Now so far as I have been able to observe the smoke nuisance from manufacturing establishments or from any given smoke stack decreases inversely with the square of the distance from the point from which the smoke issues, and I think the line where smoke is a persistent evil can be defined by a certain vegetation. I have noticed evergreens, for instance Norway pines, at a certain distance from iron works will not live. Beyond that point they will live. Now Allegheny Parks during the worst of the smoke epidemic were within that area in which evergreens would not live, but there were other areas where they were living. In the business parts they would not live.

In the lower parts of the city here the smoke nuisance in the business portion of the city, does not come from the great manufacturing establishments, in my estimation. It is from the steam engines, boilers in large buildings, used for running elevators, etc., candy factories and things of that class, right in the city limits, and we should confine ourselves, in my estimation, to the abolishment of smoke under such boilers. Our manufacturing establishments, with one or two exceptions, are so located as to cause very little annoyance to the beautiful complexions of our lady friends. I think nearly all of the ladies live in parts of the city where the smoke nuisance from the manufacturing establishments will not injure them, and I think they should understand that point. The distinction between smoke and soot is what I seek to establish.

During the advance and growth of the city in the last few years a great many steam engines and boilers have been introduced into buildings, and if we now return to coal all over the city we will have a very much worse condition than we had before if some regulation is not attempted. I think we ought to consider this question and try to abolish smoke from these buildings in the heart of the city. The city should be districted, and that is the point I wish to bring out. If the city were properly districted some ordinance could be made to abolish the smoke within these districts which would be very satisfactory to the community.

MR. ROBERT L. WALKER: I have some drawings here that I would like to show and explain. One is of a locomotive, the other is for stationary boilers. (Mr. Walker here exhibited his drawings and explained the same.) This drawing of a locomotive I built in Taunton some twelve years ago. We ran it on the Old Colony Road, the Eastern Road, Boston & Maine, the Illinois Central, the Rock Island & Pacific, Atchison, Topeka & Santa Fe. It was tested for evaporation against the best engines on the Illinois Central, and showed 8½ pounds against their engines' 4½ pounds, and on another engine on which it was tried the saving was one ton for every 70 miles ran.

Mr. Langley spoke about a double furnace. It is called the Twin Furnace. This water leg is the centre of the furnace, having a fire on each side of it. The damper here is held in place by a cap and is turned backwards and forwards by steam. If the damper was down on this side (illustrating on the drawing), the green coal was put on here and passing around through this throat goes over the incandescent fire, then up and through the flues. This damper is hollow, a continuous pipe as it were, over the water leg. The steam made in the water leg is discharged up through the crown sheet and through these pipes at the side. You see there could be no trouble with the fireman making mistakes. The operation is simply in changing the current from right to left. I used this on stationary boilers in Cincinnati, in Boston, in New York, but it has gone out of date for the reason that it is too expensive.

Here is a simple arrangement. (Illustrating from the drawing.) In the top of the arch is built a thin wall to prevent the smoke from passing backwards. The grate we divide into three parts; here is an incandescent fire, here a partially coked fire and here a green fire. The coke is pushed back in here and fresh coal is put on in the front. The air mingles with the carbon liberated from the fresh coal, passes over the incandescent fire kept up on the rear grate, the air becomes heated to a temperature of not less than 800 degrees, indeed it gets a great deal higher. It will probably reach 3000 degrees. It gets so hot that I have melted glass ten feet from the bridge-wall, which takes 2300 degrees of heat. I have melted cast-iron on top of the bridge-wall. This is the simplest invention that has ever been put on the market as a smoke consumer. There is nothing to it to get out of order but what can be replaced with little trouble and expense. It has been running in the Fall River Print Works (Mass.), for over two years with a saving of 15 per cent. They were running with 15 boilers; when I went there last year they ran with 10.

MR. M. J. BECKER: Where is that locomotive now?

MR. R. L. WALKER: That locomotive has been changed over to a common engine. It was on the Atchison, Topeka & Santa Fe Road. The doors were too small and the men could not get in to make alterations or repairs, so they turned it into a common engine.

MR. A. DEMPSTER: Are there any stationary boilers of this device convenient to Pittsburg?

MR. R. L. WALKER: At the Pennsylvania Company's offices on Penn Avenue there are four; at the West Penn Hospital there are three; at the Spang Steel & Iron Company there are sixteen, and at the Duquesne Traction Company there are three.

Mr. T. P. Roberts: You state the heat was 3000 degrees at the end of the boiler?

Mr. R. L. Walker: At the rear end of the boiler it was 2300.

Mr. T. P. Roberts: You melted iron at the bridge-wall?

Mr. R. L. Walker: Yes, sir. A puddling-furnace can be built that will not smoke but when it is wanted to do so. The blacksmith who was using coal in the time of Edward II. was told that if he made smoke his property would be confiscated, and if he did it the second time his head; and he did lose it. That is a historical fact. If all the men making smoke in Pittsburg were treated that way, there would be no smoke and no men.

Mr. D. Ashworth: I would ask the speaker as to that stationary boiler; are there any of these in any of our iron or steel works?

Mr. R. L. Walker: In the Spang Steel and Iron Works there are sixteen.

Mr. D. Ashworth: That is the very plant I referred to when I said I could see the smoke a mile away.

Mr. R. L. Walker: I have seen them run for three hours, and not a particle of smoke came out of the furnaces. The making of the smoke was the fault of the man, and not of the furnaces. Mr. Bailey says, you do not pay your men enough to get brains for running your furnaces. The firemen in this part of the country are, as a rule, laborers. I say, give firemen more money, and let the firemen be fined who make the smoke. What does it amount to the mill-owner to pay a small fine; but if it is taken out of the pockets of the working-men they feel it—their families feel it. Fine the fireman once or twice, and you will find that there is no smoke coming out of your furnaces. I have seen the boilers smoke at the Spang Iron and Steel Company when they were trying to urge the boilers—making eight boilers do the work of fourteen.

Mr. M. J. Becker: Do I understand you to say this improved boiler-furnace was in use in the Pennsylvania Company's building on Penn Avenue?

Mr. R. L. Walker: There are four of them in there now.

Mr. M. J. Becker: I cannot see from here distinctly, but, whatever is the apparatus recently introduced in the boilers of that building, I know the smoke has been very materially reduced since its introduction. I was told by the fireman to-day, and my visit to the cellar was quite accidental, that the amount of fuel consumed was reduced from 15 to 20 per cent. These boilers are ordinary heating boilers. That is, they are used to heat the building, and, of course, the steam-pressure is very low.

MR. R. L. WALKER: They carry about 60 pounds, and heat the building and run the elevator.

MR. THOS. H. JOHNSON: They run a pump, but not the elevator.

MR. P. BARNES: In this connection I would say, that Mr. Koch testified to me in the strongest terms of this device. Whether it makes smoke or not, is a little past the mark, but his testimony was very emphatic.

MR. PORTER: I have nothing to say in particular, further than that we have given this smoke-consumer a pretty fair trial. We first put in two of them, and ran them about a week. We tested them in every way they could be tested. We were so well satisfied with the extra producing of steam that I ordered in fourteen more at once, and we are running now sixteen.

I will agree with Mr. Ashworth, that we occasionally make smoke. He has seen it, and I have seen it, myself. I have gone into the mill and have seen more smoke than there ought to be, and I have told the superintendent that it should be stopped. He went into the boiler-house and stopped the smoke as soon as he got the firemen working right. We must have men that have brains.

But as to this device, I do know it makes a considerable difference in the amount of our coal bills a month. I think there is a saving of at least 20 per cent.

MR. D. ASHWORTH: If there is any one point that has pleased me to-night, it is the awakening to the fact that we want more intelligence at the shovel-end of the boiler. I have advocated that for years. It is a well-known fact to everybody, that the general impression is, that anybody is good enough to shovel coal under a boiler. There never was a greater mistake. You are losing your heat up the stack, and I am glad to have received some experience to-night.

One of the papers read at the Buffalo meeting of the National Association of Electrical Engineers was, how to fire under boilers, and, in the discussion of that, this very same subject was talked upon, for it is not a local matter, it is a national idea. We are just upon the threshold of having just as much intelligence at the shovel-end as at the throttle, or further on, at the counting-room. I will allow no man to go farther in this direction than myself. I have advocated it for years; but I will just repeat one thing. While I admire the stoking principle, I believe it is correct, and we can abate this smoke down almost to the minimum, the moment you begin to overurge your boilers and stir the fires up, then you will destroy the object sought.

Mr. Walker: I was in Chicago two weeks ago at a meeting of the Master Mechanics Association before the City Commissioners. The Board brought them there to talk about the smoke from the engines. They have all been using the steam-jet. All the experience has been that the steam-jet does ruin the boilers. The top sheet of the locomotive only lasts from two to three years, and it cracks. You cannot give the steam-jet in Chicago away. They won't have it.

Mr. Dempster: We have had all the patents and furnaces ventilated here to-night. We have been given a general exhibition by the patentees which is beneficial to the public, no doubt, as it is interesting to us, but what will we do about it? How will it benefit us? What is the practical application? It concerns these ladies. It concerns this Society. It concerns everybody to know how the means are to be applied. What will reduce the distribution of smoke? The Engineers' Society can talk about it, but the powers at City Hall must make the application. I would make a motion to investigate and get facts, and from these facts collate such data as will be of benefit to the citizens of Pittsburg. It occurs to me that is the only way in which we can get at the best results. This gentleman can produce the Roney stoker as the panacea of the smoke ill, and the other the Murphy; this man some other and that man still another, but they cannot make positive any certain conditions. They can produce certain results under some conditions, but that does not fulfil all the requirements.

What the city of Pittsburg and everybody else wants to know is, *the most practicable and efficient means to minimize the production of smoke.*

It occurs to me that the better way to get at results would be for a few men, say a committee, to make an investigation to get all the facts. If these stoker gentlemen wish to submit their data, well and good. I think that would be more effective in getting results than all the talking and assertions that we can make in our Society.

MR. SCHELLENBERG: There is one idea that has not been suggested, and that is that coal and gas will burn together and reduce the smoke. I have seen that tried. The gas dries up the coal; the coal burns down to fine ashes. There is a great deal of heat produced, but no smoke.

JOSEPH EICHBAUM (Communicated): In view of the fact that many users of steam are returning to the use of coal, and believing that much of the smoke is unnecessary as well as wasteful, I offer my experience.

Some years ago I was using a small flue boiler 16 feet by 36 inches for power, and an Eclipse boiler for heating, the two boilers using 42 bushels of coal per day.

The flue boiler proving inadequate, I changed the furnace by enlarging it, and made ample steam for both purposes, with a consumption of 18½ bushels per day.

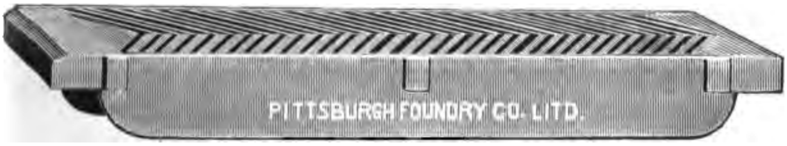
I afterwards reset the same boiler, giving a space of 8 inches between the boiler and the walls instead of 4 inches, as is customary, and produced the same results with 15½ bushels.

Another boiler, 22 feet by 38 inches, with two 14-inch flues, was treated the same way, excepting that the space at the sides was too small, and could not be enlarged on account of the surroundings. It had used 45 bushels per day, and was brought down to 21. This was ascertained by weighing the coal six days before the furnace was changed and six days after. Originally, the grate surface was 14 feet, which was increased to 21 feet. The grate surface of the first boiler mentioned was 9 feet, which was increased to 20 feet. It is probable the kind of grate bar used had something to do with it.

I had been using straight bars, four sets a year, but changed to bars of 6-inch face, with flanges 5 inches deep on each side. The faces of these bars are perforated with diamond-shaped openings. They were in good condition after eleven years' use.

JOHN McDONALD (Communicated): The grate bar referréd to by Mr. Eichbaum is known as the "Diamond" grate bar. It is cast in different lengths to suit the furnace. It is 6 inches wide. It has a flange on each side, the full length of the bar, about ½ inch thick, and projecting below the surface of the bar about 5 inches. These flanges add strength and prevent warping. The air-openings in the bar are diamond-shaped, and are made of different dimensions to suit different sizes of coal.

A similar grate bar is known as the "Herring-bone" or "Tupper-bar." It is cast the same as the "Diamond," excepting that the air-openings are V-shaped. Both of these are good bars for burning slack.


PITTSBURGH FOUNDRY CO. LTD.

Tupper Grate Bar.

J. H. HARLOW (Communicated): As a matter of interest, I might say that in the spring of 1891, being in Philadelphia, and interested in the probable introduction of some smoke-consuming apparatus near Pittsburg, I visited the Spreckels Sugar Refinery, where they have a combined capacity of 7500 horse-power boilers on two floors. At the time of my visit they were using from 25 to 30 car-loads of Cumberland coal per day under these boilers, and the total labor employed was three men on each floor (six in all) to fire and watch the steam and water, and one man to take care of the ashes. To be sure, they had the mechanical methods of handling the coal. The products of combustion passed out through two stacks, and, as a rule, they were smokeless. Occasionally, however, smoke would pass out through the top. This was said to be when the men were using their slashing bars, and had their stoker-holes open. I was so well satisfied with this stoker that I purchased one for our own work,—not so much for the sake of the saving in coal (which, however, we hoped would amount to something), as a probable saving in labor and the annoyance from smoke.

I agree with Mr. Ashworth in the idea that the stoker-fired boilers do not respond quickly on sudden demands, nor are they specially advantageous when the boiler is driven greatly in excess of its rated capacity. From the records kept thus far by the Wilkinsburg Electric Co., I do not perceive that there is any special saving in the amount of coal consumed per pound of water evaporated, but there is a considerable saving in labor required, and, as a general thing, but little smoke issues from the chimney, except when the fireman opens the stoker-holes.

At the last meeting there were some remarks made by Mr. Koch in relation to Birmingham, England. I recently met a friend of mine, Mr. Thomas Addenbrook, of the Edgar Thomson Steel Works, who has just returned from a visit to his old home at Birmingham, England, and I submit a copy of a letter received from him in reply to an invitation from me to attend our meeting and take part.

PITTSBURG, MARCH 14, 1892.

MR. J. H. HARLOW.

Dear Sir: In reply to your invitation to attend the meeting of the Engineers' Society, I regret to say that another engagement prevents my acceptance. I do not know whether anything I could say would prove of interest to you, but, on my recent visit to Birmingham, England, I was surprised to see such a clear atmosphere and smokeless chimneys compared with the Birmingham I saw twenty-one years ago. Yet one would expect to see more smoke when we remember that the climate is so much damper than our own. A gentleman connected with one of the large dry-goods stores on Corporation Street, Birmingham, England, told me that he had been informed it was almost impossible to purchase a piece of light colored goods, unsoiled, in Pittsburg, on account of the dirt and smoke. I gave him the best answer I could. He replied that that had never been said of Birmingham, they watched their chimneys too close. I am sorry I did not look more closely into the matter, but might say that during the four or five days I was in the city, I saw no chimney with smoke pouring out of it.

Very respectfully yours,
THOS. ADDENBROOK.

On motion of Mr. Dempster, the chair appointed the following Committee on Smoke Prevention: Alex. Dempster, *Chairman*, Thos. H. Johnson, C. Hyde, W. L. Scaife, J. W. Langley.

Adjourned.

R. N. CLARK,
Secretary.

MEETING OF THE CHEMICAL SECTION OF THE ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

MARCH 24TH, 1892.

Meeting was called to order by Prof. Langley. Twenty-five chemists present.

Minutes of last meeting were read and approved.

Report of committee No. 1 was read by James O. Handy.

The formation of a Chemical Section of the Engineers' Society had been approved by the Board of Directors at their meeting, March 12, 1892, and by the Engineers' Society at a regular meeting, March 15, 1892.

Report of committee No. 2 was read by Mr. Van Gundy. It showed the conditions and advantages of membership in the American Chemical Society.

Several letters were read from members of the American Chemical Society asking the co-operation of Pittsburg chemists.

Fifteen new applications for membership in the chemical section were received. The total number of applicants to date were fifty. There were known to be fifty other chemists in the vicinity who ought to help and be helped by the society.

A motion was made by Mr. Handy "that the chemists join the Engineers' Society as individuals and subsequently organize a section for independent meetings."

After some discussion this motion was put and carried unanimously.

Mr. Handy then read his Paper on

A RAPID METHOD FOR PHOSPHORUS DETERMINATION IN IRON, STEEL, AND ORES."

BY JAMES O. HANDY,

CHEMIST OF THE PITTSBURG TESTING LABORATORY.

No method of determining phosphorus will find favor among chemists unless it possesses advantages over existing methods. While retaining the accuracy of the best methods now known, it must be simpler, more rapidly executed, or less expensive. My belief that the process which I have worked out is a step in this direction, is my reason for now describing it.

I owe to Mr. C. E. Manby's paper, in the J. A. & A. C. for February, 1892, the suggestion of essential points in my process. The method which he gives is too long, and would not compete with the Emmerton-Drown-Jones method (A. I. M. E., February, 1890), which can be executed, with proper appliances, in thirty minutes, or less. Mr. E. F. Wood's latest method, a gravimetric one, is said to yield results in twenty minutes. I do not know how uniformly reliable these results are. I believe that they are always checked by a longer method.

I have proved by a great many experiments, that several of the steps of Mr. Manby's process can either be safely dropped, or simply modified, and a rapid and satisfactory process produced.

My method consists of the following points:

(*a*). Separation of the phosphorus as *pure* phospho-molybdate of ammonium, by washing with neutral potassium nitrate solution, after the regular washing with 1 per cent. nitric acid.

(*b*). Solution of the pure phospho-molybdate in a measured volume of a standard sodium hydrate solution. A definite quantity of the alkali is taken up in neutralizing the phospho-molybdate.

(c). Titration of the excess of sodium hydrate by means of standard nitric acid. Phenol-phtalein is used as the indicator. The end reaction is very distinct. Duplicate analyses give closely concordant results, which agree exactly with determinations of phosphorus by gravimetric methods. Without any special appliances, and without use of suction, an analysis can easily be done in thirty minutes. I am confident that this time can be shortened one-half. An analysis has in fact just been done in seventeen minutes. (March 29th.)

The first part of the process is essentially that of Dr. Drown (A. I. M. E., June, 1889). I have confirmed, by long experience, Dr. Drown's statement that sugar can be safely used as a reducing agent in the process. Clemens Jones substituted ferrous sulphate, but I find that it requires several times as much ferrous sulphate as sugar for reduction, and that the sugar produces no ill effects. Pure granulated sugar is used.

THE METHOD.

Dissolve 2 grammes of steel in a 12-ounce Erlenmeyer flask, in 75 c.c. of nitric acid (1.13). To the boiling solution, add 15 c.c. of permanganate solution (5 grammes per liter). Boil until pink color disappears. Oxidation of the carbon and phosphorus is complete if there remains a brown precipitate of oxide of manganese. More permanganate will be required only in the analysis of iron or steel of very high carbon. Remove flask for a moment from the light. Add about $\frac{1}{30}$ gramme of granulated sugar. Replace flask on the light and heat till the solution clears. Take flask off the light, and, after two or three minutes, add 10 c.c. of .90 ammonia. If the latter is poured carefully down the side of the flask, no loss by spattering will occur. Agitate the flask-contents till, in a moment, the ferric hydrate is re-dissolved. Insert thermometer, and cool (or warm) to 85 degrees centigrade. Add 50 c.c. of molybdate solution, of temperature about 20° C., causing it to rinse off the thermometer as it flows into the flask. Insert rubber stopper. Wrap flask in towel, or put it into a shaking-box, and shake it for five minutes. Filter immediately, using a 9 c.m. Swedish filter, and moderate suction. The precipitation is complete, and the precipitate shows no tendency to run through the filter. Wash out flask, and wash the filter and contents five times with 1 per cent. nitric acid; wash five times with $\frac{1}{10}$ per cent. potassium nitrate wash (1 gramme per liter). Place filter and contents in the flask. Add from a pipette 10 or 20 c.c. of standard sodium hydrate solution. Shake for a moment, until the yellow precipitate is dissolved. Dilute with water to about 50 c.c. Add 3 drops phenol-phtalein solution and titrate with standard nitric acid till the pink color disappears. The cubic centimeters of standard sodium hydrate used up in neutralizing the yellow precipitate represent the hundredths per cent. of phosphorus in the sample analyzed.

REAGENTS.

Molybdate Solution.—E. F. Wood's 1888 formula.—1 pound of molybdic acid is mixed with 1200 c.c. of water in a stone-ware jar; 700 c.c. of ammonia (.90 sp. gr.) are stirred in, and the stirring continued till all soluble matter is dissolved. 300 c.c. of conc. nitric acid (sp. gr. 1.42) are added to partially neutralize the ammonia in the solution. In each of four 2½-liter bottles is placed a mixture of 500 c.c. nitric acid (sp. gr. 1.42) and 1200 c.c. of water.

Pour 550 c.c. of the molybdate solution through a funnel into each bottle, and mix the contents by a slight rotating motion. If the stream of molybdate solution flows quickly and continuously into the acid mixture, no separation of molybdic acid takes place. Both solutions may be hot when mixed.

I have used this formula for three years with uniform success, and have seen it in continuous use in many other laboratories. Little or no separation of molybdic acid takes place on standing, but we always filter it just before using.

FILTER PAPER.

No. 1 "F." Munktel's Swedish filter paper has given us entire satisfaction. We are much pleased with its uniformity.

Nine c. m. filters are used.

PURE YELLOW PRECIPITATE.

This can be prepared from acidified ammonium or sodium phosphate by precipitating with molybdate solution. Wash the precipitate with 1 per cent. nitric wash. Dry at 100° Centigrade. Keep in glass-stoppered bottles for use.

Use (one-tenth gramme) for standardizing the sodium hydrate solution for work on steels, etc., containing from .050 to .150 per cent. of phosphorus. For high phosphorus work use a quantity of phospho-molybdate approximately equal to the amount obtained from the analysis of 2 grammes of the metal. This precaution avoids multiplying the slight excess of nitric acid which is added when titrating excess of sodium hydrate. This excess would be one drop or perhaps one-tenth c.c., and would become significant if multiplied by five or ten. By observing this precaution the accuracy of the method may be preserved through a wide range of phosphorus percentage.

When the method is once established in a laboratory, the standardization of the sodium hydrate solution may be more conveniently done by means of steel or pig-iron of known phosphorus contents.

STANDARD SODIUM HYDRATE.

15.4 grammes of sodium hydrate are dissolved in 100 c.c. of water. Saturated barium hydrate solution is stirred into the sodium hydrate solution until no more precipitation occurs. Filter at once and make up to two liters with distilled water. In practice, I shall make up 154 grammes of sodium hydrate at once, freeing it from carbonate as above and make up 2 liters. 200 c.c. of this, made up to 2 liters will make the standard approximately.

STANDARD NITRIC ACID.

For a *stock solution*, make up 200 c.c. of nitric acid (sp. gr. 1.42) to 2 liters. 200 c.c. of the stock solution is made up to 2 liters for approximate standard. Run the standard alkali and acid against each other, and having ascertained their relative strengths, add sufficient water to the stronger of the two, to make the solutions equal in value.

Next run the sodium hydrate solution against .1000 gramme pure ammonium phospho-molybdate = .00163 gramme phosphorus. If its strength is too great (*e.g.*, if 1 c.c. = .000206 gramme phosphorus) add to 2 liters of the standard sodium hydrate 60 c.c. of water. In order to preserve the equality of the acid and alkali solutions, add to 2 liters of the standard nitric acid 60 c.c. of water and mix. The solutions will then be equal and 1 c.c. = .0002 gramme of phosphorus or $\frac{1}{100}$ per cent. phosphorus, when 2 grammes of the sample are taken for analysis.

PHENOL-PHTALEIN INDICATOR.

Five-tenths gramme of phenol-phtalein are dissolved in 200 c.c. of 95 per cent. alcohol. Use about 3 drops for each titration.

We feel sure that the phospho-molybdate of ammonium, as we obtain it in this process contains, when dried at 100° Centigrade, 1.63 per cent. of phosphorus. Our reasons are as follows:

1. Mr. E. F. Wood finds 1.63 per cent. phosphorus in the phospho-molybdate obtained by his method and dried at 100° Centigrade. (See J. A. C., 1888).

2. We have found by hundreds of analyses the absolute agreement of results by Mr. Wood's process with those by Dr. Drown's (A. I. M. E., 1889) process, modified by *weighing* instead of *titrating* the yellow precipitate.

3. Dr. Franz Hundeshagen, in a very elaborate series of researches (Z. A. C., 1889), finds that phospho-molybdate precipitated and purified, as we do it, has 1.63 per cent. of phosphorus when *dried at* 150° *Centigrade.*

4. We find that pure phospho-molybdate *dried at* 100° *Centigrade* suffers no further loss of weight when *dried at* 150° *Centigrade.* Hence we have two authorities to prove that the phospho-molybdate as we obtain it contains 1.63 per cent. phosphorus.

5. We find that results obtained by precipitation at temperatures from 20° to 85° Centigrade are identical.

6. We find that samples precipitated at 85° Centigrade can be filtered immediately and correct results obtained, while at lower temperatures longer standing is needed before precipitation of the phosphorus is complete.

7. We find by experiments on a sample of steel containing .014 per cent. of arsenic that the phosphorus can be correctly estimated by our method without added precaution.

8. We find perfect agreement between results by weighing the yellow precipitate and results by titrating the same by our method.

Tables of results of some of the experiments by which we established the accuracy of the method are given below:

No.	Results by weighing phospho-molybdate. Per cent. phosphorus.	Results by titrating phospho-molybdate. Per cent. phosphorus.
4289,	.093	.093
4290,	.089	.088
4291,	.096	.095
4292,	.038	.039
3143,	.037	.038
A,	.090	.089
3211,	.088	.087
3212,	.090	.089
3214,	.033	.033
3213,	.089	.091
3216,	.088	.087
4288,	.071	.072
152,	.425	.422
4327,	.030	.030
Blair,	.098 .097 .097	.098

The last sample contains, according to Messrs. Booth, Garrett, & Blair, phosphorus .097 per cent., arsenic .014 per cent.

DISCUSSION.

MR. MANBY: I am very glad, indeed, to see that my method has been so quickly taken up and improved by Mr. Handy, and that he has obtained such good results with it. I hope that the investigations of your committee will confirm his results. I have made estimations with the method in forty minutes, and I am pleased to see that this time has been cut down one-half by Mr. Handy. I found that the presence of ammonia salts was prejudicial to the accuracy of the titration, and I therefore resorted to the process of purifying the yellow precipitate by igniting it moderately over a lamp to expel ammonium nitrate. I consider the freeing of the yellow precipitate from acid and ammonium salts, by washing with $\frac{1}{10}$ per cent. potassium nitrate, a material improvement. I had feared to attempt titrating, with filter-paper suspended in the solution, but Mr. Handy's good results show that this fear was ungrounded.

The reaction which takes place when the sodium hydrate solution is added to the yellow precipitate is an interesting study. I think that the yellow precipitate has the composition $(NH_4)_3PO_4$, $12MoO_3$. I believe *that the ammonia* in this compound *is replaced* by soda when excess of sodium hydrate is added. I think that with the conditions of dilution which we operate under, the ammonia would be held in solution.

MR. VAN GUNDY: I believe that the soda *does not replace* the ammonia; for, if it did, then an exactly equivalent amount of ammonia would be set free, and the alkalimetric value of the resultant solution would be identical with that of the measured amount of soda solution which was added. As practice shows that this is not the case, but, as the amount of soda used up varies with the amount of yellow precipitate present, I believe that the base of the soda unites directly with the phospho-molybdate of ammonia to form a neutral compound.

MR. HANDY: I favor Mr. Van Gundy's theory.

MR. MANBY: I believe that there is a certain amount of nitric acid which, ordinarily separates out with the yellow precipitate, and *is not* removed by drying at 100 degrees Centigrade, but *is* expelled by higher heat.

MR. HANDY: My experience and study do not let me believe in the presence of nitric acid in the yellow precipitate, either (*a*) when washed with 1 per cent. nitric acid, and dried at 100 degrees Centigrade, or (*b*), when washed with 1 per cent. nitric acid, and then with $\frac{1}{10}$ per cent. potassium nitrate, and not dried. I prepared pure yellow precipitate by washing with 1 per cent. nitric acid and drying at 100 degrees C. This material was placed in glass-stoppered bottles to prevent reabsorption of moisture. Portions of this yellow precipitate exposed for a half-hour to temperatures from 135 degrees to 165 degrees Centigrade in an air-oven, no loss of weight was found in either case.

In regard to the presence of nitric acid in the yellow precipitate after washing with 1 per cent. nitric acid, and then with $\frac{1}{10}$ per cent. potassium nitrate, I do not believe in it for the reason that almost identical results are obtained by titrating by my process the undried and the dried precipitates. Slight variations were occasionally obtained, but I found a higher value for the dried than the undried precipitates. These two facts, I think, effectually disprove the presence of nitric acid in the yellow precipitate.

We have, further, the statement of Dr. F. Hundeshagen, in his most elaborate research (Z. A. C., 1889), that the yellow precipitate obtained by precipitation under conditions which are the same that we have in our method, has the composition $(NH_4)_3,PO_4, 12MoO_3$, when dried at 150 degrees. No nitric acid is in the formula.

MR. EASTWICK (to Mr. Manby): Do you not consider the precaution of making your standard sodium hydrate solution free from carbonate of sodium a very necessary one?

MR. MANBY: I do; and I give Mr. Handy credit for this good suggestion.

MR. CARNAHAN: I have found slight loss of weight to take place when yellow precipitate, dried at 100 degrees Centigrade, is exposed to a temperature of 150 degrees Centigrade. The lost did not amount to more than 1 per cent. of the weight of the yellow precipitate.

MR. C. M. JOHNSON: I also have found a loss of weight when the yellow precipitate, dried on a filter at 100 degrees Centigrade, is exposed to 150 degrees Centigrade.

MR. HANDY: I believe that Mr. Carnahan's and Mr. Johnson's losses of weight were due to the oxidation of the carbon of the filter at the expense of the oxygen of the molybdate. This action is evidenced by the progressive blueing of the filter and precipitate when exposed longer than three-quarters of an hour to a temperature of 100 degrees Centigrade or over.

My experiments were made by heating pure yellow precipitate in porcelain crucibles. I thus avoided any error caused by filter-paper oxidation.

After discussion of this a committee consisting of Messrs. Crabtree, Kemery and Camp volunteered to investigate the method and report results at next meeting.

On motion of Mr. Jones a committee was appointed "to draft such constitution and by-laws as they might consider necessary for the proper conduct of the affairs of the chemical section." Messrs. Jones, Langley and Handy were appointed.

After filing applications for membership, and appointing April 21st as the date of the next meeting, the chemists adjourned.

April 19th, 1892.

The Society met in the parlors of the Academy of Science and Art, on April 19, 1892.

Mr. Alfred E. Hunt, President, in the chair, Mr. R. N. Clark, Secretary; forty-five members were present. The minutes of the last meeting were read and approved. The following gentlemen were elected members of the Society by ballot: Ferdinand Andrews, James Brakes, J. C. Barrett, E. S. Batchelor, W. A. Barrows, Jr., Charles Castrique, Frank L. Crobaugh, W. H. Coster, Frank I. Cooper, Warren R. Clifton, Ferd. Crabtree, C. H. Dickson, W. H. Daly, M.D., Charles B. Dudley, M.D., Arthur V. Davis, E. L. Elliott, Joseph H. Eastwick, George Faunce, John Floyd, S. A. Ford, N. B. Graham, Thomas D. Gill, George G. Glass, E. B. Hull, John G. Hall, J. A. Hope, Jr., W. Harrison, M.D., John S. Harvey, C. M. Johnson, R. G. Johnston, Philo Kemery, Samuel A. Kennedy, G. G. Kirby, C. B. Lihme, George O. Loeffler, Robert Miller, George A. Marsh, Uriah H. Myers, Charles T. Mixer, Jacob A. Mohr, J. K. Moorhead, J. P. Murray, Charles E. Manby, James P. McKelvey, Alex. G. McKenna, Charles J. Pedder, F. N. Pease, Irving A. Palmer, F. G. Ross, F. S. Slocum, Karl F. Stahl, Wm. Aubrey Thomas, C. W. Townsend, S. S. Voorhess, C. P. Van Gundy, Philip R. Whitman, Fred. K. H. Williams, John E. Williams, Joseph M. Wilson, Edward F. Wendt, Henry T. Weed, E. F. Wood.

The Library Committee reported that fifty-four bound volumes had been added to the Library since the January meeting. The

Committee on Smoke reported through Prof. J. W. Langley, that the Committee thought it advisable to organize themselves into a number of sub-committees, each of these sub-committees to take a certain feature of the subject as their special function, the entire committee to hear the reports of the sub-committees and in the course of time to make a full report to the Society.

President Hunt: I might add to what Prof. Langley has said that the Committee will be very glad to receive any documents or suggestions that can be taken into consideration in their work. Any such communications should be sent to Mr. Alexander Dempster, whose office is 535 Wood Street, Pittsburg.

The Committee on "Water Ways" reported through:

Mr. Thos. P. Roberts: I did not know that I was a Committee. I received the documents sent to this Society and referred to me by our Secretary, and I believe that I have been requested to prepare a paper for the Congress which will meet in July in Paris. This Congress meets at intervals of two years, the last meeting having been at Manchester. This year the meeting is in Paris. They expect to have another meeting in Chicago next year which will be represented more distinctly by American engineers. I am in hopes to be able to prepare the paper desired, and I will tell the Society about it if I make any progress.

Mr. George S. Davison then read his Paper on

DISCHARGE OBSERVATIONS OF LARGE STREAMS.

Among the many problems that from time to time present themselves for solution to the engineer, are those which relate to natural streams of water. It may be necessary to know, at least approximately, the amount of water discharged by a certain stream within a given time, the amount of sediment in suspension carried along by its waters, and perhaps the direction of its current at various places. A knowledge of all these points is particularly necessary in the cases of contemplated improvements of our large western and southern rivers, flowing, as they do, through long stretches of alluvial soil, and possessing an innate desire to swallow up the sites of whole villages and towns along their banks, or to make a short cut across a narrow neck of some peninsula that they had formed years before, leaving large commercial interests that have been depending upon them away out in the cold. The ease with which these streams will swallow up a man's farm almost in a night, or by a cut-off, transfer him farm and all from the State of his allegiance to one whose laws may not be to his liking, and possibly transfer him back after a few years, is simply astonishing to an observer, and somewhat wearisome to the man with the farm.

Velocity and discharge observations have been made upon a great many streams in connection with questions of water supply or improvements of their water-ways for navigation, but to the efforts of our own government to subdue the unruly streams above mentioned, and to cause them to flow within their proper confines, and rid themselves of shoals that are impediments to navigation, is due the credit of the most extensive experiments in this direction. Indeed, the expense attending such investigations is so large that no private enterprise can engage in them to any but a very limited extent. Just what light some of these very extended investigations have thrown upon the subject will be discussed farther on.

In general, the difficulty of investigating the questions involved is greater as the stream is larger. In the case of a very small stream, the quantity of water passing along its channel may be so small that it can be entrapped in vessels of known capacity and thus measured, or, if too large for such proceeding, it may be made to pass through one or more orifices or pipes, or over a weir artificially constructed, and by means of well-known formulæ, into which the quantities of observation are introduced, its discharge may be very closely approximated. It is not the purpose of this paper to deal with streams that can be so treated, but only those so large that their natural section cannot conveniently be changed to an artificial one of regular form.

The quantity of water passing any section of any stream in a unit of time is given by determining the quantities in the second member of the very simple equation of

$$Q = Av,$$

in which A is the area of the section under consideration, and v the mean velocity of the water at that section for the unit of time, generally one second. The equation looks very innocent, and it is so far as the quantity A is concerned; but the treachery of that little letter v has kept hydraulicians busy for the past two hundred and fifty years. In the seventeenth century, Castelli, Torricelli, Marriotte, Gugleilmini, and others, in their order named, wrote upon the subject; the first named introducing the velocity as an element in calculating the discharge; the second discovering the analogy of the velocity of water flowing from orifices of vessels to that of falling bodies, falling through distances equal to the depths of the orifices below the surface of the water in the vessels; or in other words, that the formula, $v = \sqrt{2gh}$, could be applied in determining the velocity of the issuing water. Mariotte made numerous experiments in connection with the subject of velocity, but developed nothing new; while Gugleilmini, in adopting the Torricelli theorem, proclaimed the fallacious theory that the particles of a fluid current would tend to move with velocities proportional to the squares of their depths below the surface. About 1730, Pitot, by experiments with the tube which has since borne his name, demonstrated the error in this theory. However, nothing tangible in the way of formulæ based upon extended experiments and expressing the value of v was evolved until about one hundred years ago. Then Chezy, Dubuat, De Prony, Young, and several others, within a period of about twenty-five years, determined the various formulæ which bear their respective names. They each and all express the velocity in terms of the cross-section and slope, or, to be more specific, their various formulæ, by neglecting a few small constants in some of them, may be written under the general form of $v = C\sqrt{rs}$, in which r is the mean radius or depth, found by dividing the area of the section by its wetted perimeter, s the fall per foot of the surface, and C a coefficient to which was assigned values derived from experiment. To C some authors of that time gave constant values; others, values dependent upon rs; and still others made it to depend separately on r and s. This value, as assigned by different experimenters, was as low as 69 and as high as 100. The general form of the equation was based upon theoretical reasoning, C alone, both in form of expression and value, being determined by experiment. In general, the basis of reasoning was founded on the principle that in cases of streams of uniform motion, the component of weight of the water parallel to the surface, and which tended to increase the velocity, was just equalled by the resisting forces, such as the friction on the bottom, and thus the state of uniform motion was maintained. By equating the expression for the accelerating force with one for the resisting force, the general value of v as given above was found, and then experiments followed to determine C. These experiments were conducted generally upon small streams, both natural and artificial, but did not cover a wide range of conditions under any one experimenter, and, as it may be supposed that each one experimented under conditions varying greatly from those of others, they thus found widely different values.

As near as I can learn, the first observations on velocity of large streams were taken about eighty years ago by Krayenhoff, on some of the rivers of Holland, and they have been followed, at various times, by velocity observations on most of the large rivers of all countries. The most complete and extended surveys of this kind were those of Humphrey and Abbott, made at several points on the Mississippi river in the years 1851 and 1858, a full account of which, in addition to a mass of information on the general subject of river hydraulics, is found in their admirable book of *Report on the Mississippi River.* From their data, obtained in these observations, they established some very important truths, which shall be alluded to farther along, but the main conclusion of all their experiments is summed up in an entirely new formula for the expression of the mean velocity of rivers in general. Their value of v for large streams is $v = \sqrt{(.0015 + \sqrt{225\, r_1 \sqrt{s}} - .03879)^2}$. As a means of comparison with the formula given above, their formula would, by omitting certain small quantities, be written $v = C\sqrt{r}\sqrt[4]{s}$, C being a variable co-efficient, dependent on r, and s being the mean radius obtained, not by the quotient of the wetted perimeter into the area of section as before, but the *total* perimeter. They apply a set of tests to their own and other noted formulæ for mean velocity, by comparisons with the mean velocity as obtained by float observations by various observers upon a number of large streams. In the comparative table of results it would appear that their formula gives very close results and the others do not.

Up to 1865, when D'Arcy and Bazin published their researches, the question of just what effect the character of the bottoms of channels had upon the velocity of water passing along them, did not seem to have occurred to any of the previous experimenters, or at least, if it did, they did not seem to think it to be of sufficient importance to be taken into account. These two gentlemen, while agreeing with Humphrey and Abbott in some of their results, do not favor the formula of the latter for mean velocity, but adhere to the old form, and proceed to give four forms of values for C, according to the four classes of wetted perimeters into which they classify them according to their degree of roughness. In their formula, C is a function of r, and the condition of the bottom.

In 1870, Kutter appears, and, reviewing a very large number of experiments taken from the data of all observers, he reaches the general form for an equation expressing the mean velocity, which is now very generally accepted by hydraulic engineers. It is of the Chezy type, and as a matter of interest, to show how degraded v has become in all these years of rough usage, we give the Kutter formula, which is:

$$v = \left\{ \frac{41.6 + \frac{1.811}{\mathrm{N}} + \frac{0.00281}{s}}{1 + \left(41.6 + \frac{0.00281}{s}\right) \frac{\mathrm{N}}{\sqrt{r}}} \right\} \sqrt{rs}.$$

The entire quantity within the brackets is the value of C, and, as appears, is a function of r, s and N.

The terms r and s continue to represent the mean radius and the slope respectively. N, the only other quantity that must be determined to solve the equation, is dependent on the character of the wetted perimeter, and has been assigned various values from

.009 for a bottom of smooth-planed timber to .050 in very irregular beds that are covered with vegetation and rough stones; the intermediate values having been assigned to probably a dozen different conditions. If, then, this formula be accepted as the correct one for large streams, and under any condition of roughness of bed, three quantities must be obtained by observation for the purpose of calculating discharge; namely, the longitudinal slope or inclination of the surface of the stream at the point of observation, the accurate section from which the mean radius is deduced, and the condition of the bottom of that section. This again seems all very easy, and we are at last in a fair way of determining the mean velocity, and consequently the discharge, by a formula that receives the sanction of good authority. And yet, what personal experience we have had with large rivers convinces us that this formula, however accurate for small streams and artificial channels, is unreliable for use in cases of large streams.

It can be easily understood how a rough river bottom could have a very strong retarding effect upon the lowest filaments of water, both on account of the great friction between the water and the projections of the bottom and also the internal interferences of the counter-currents thus induced, and the effect may travel to a considerable distance above the bottom, thus affecting the velocities at various depths over a case of less of the roughness and consequent less friction. This idea of cause and effect is, as before stated, brought out in the Kutter formula, with its different values of N for the various classes of perimeters. However, we believe that while a set of such co-efficients might be correct for artificial conduits or a class of streams in which the mean radius is small and slope large, and the character of whose sides is clearly defined, they would not be for streams of all sizes. To add to this uncertainty of general correctness of values assigned to N, there is a large amount of guessing necessary to arrive at the particular class to which the bottoms of large streams might belong. It would at least seem necessary to establish a value for N for each particular locality.

To return to the Humphrey and Abbott survey. They made experiments to determine the relation of the velocities in vertical sections parallel with the course of the river. These were taken at various stages of the river, and under various conditions of wind, notations of which were made. Each section was plotted on cross-section paper, the depths at which the floats were run being laid off vertically as abscissæ and the velocities corresponding to the depths were the ordinates laid off horizontally. All sections observed under the same conditions of wind and stage of water were combined, and also the results at all vertical sections were combined in one exhibit. These graphical exhibits made it possible to draw thereupon a line of average velocity on each combination of observed velocities, and thus for the first time the very important truth of the relation of velocities in vertical planes was shown. Each and all of these showed the same general results and gave curves of parabolic form as shown in Fig. 1, the line S B being the average line of velocities drawn among the many points plotted from the notes. It might be remarked that a single set of observations generally showed a great irregularity and it required a number of them to be combined in plotting to determine an average line. The irregularity of position of the small circles in the figure, each representing an observed velocity, gives an idea of this. The form of this general form of the curve of velocities in vertical sections shows that the point of greatest velocity is below the surface, and that the position of mean velocity is near mid-depth. An up-stream wind naturally retards the water near the surface, thus diminishing the velocity of the water at and near it, and observations made under the condition of an up-stream wind shows the position of maximum velocity to be depressed, and those made during down-stream winds show the reverse. The lines of velocities under these conditions are shown in Fig. 2, in connection with a line of velocities during a calm. The change of position of the axis of the curves, which position is the locus of the maximum velocity, is also apparent. This position during the condition of a calm was shown to be at three-tenths of the depth.

Just why, under ordinary conditions, the surface velocity is less than that at some point beneath the surface, has never been satisfactorily explained, but that it is so, and to an extent represented in these and other observations can be practically demonstrated at any time. It was no doubt due to this that Humphrey and Abbott considered the surface in their values of the mean radius, as they likened the retardation at the surface to that at the bottom.

One prominent conclusion arrived at was that while the mean velocity of any vertical section bore no constant relation to either the surface or maximum velocities under all conditions of outside influences, yet it did bear an almost constant relation to the mid-depth velocity, and for all practical purposes the ratio was considered constant. They show from a number of observations of their own and others, that the quotient of the mean velocity divided by the mid-depth velocity varied from 0.93 to 0.98. The average of a number of observations at Fulton, which will be referred to later, gives from .992 to 1.004, the grand mean value being .997. The very accurate observations with current meters made under Major McKenzie in the Mississippi River at Burlington, Iowa, during October, 1879, gave as a grand mean of all observations, the ratio of .958.

Probably the first extensive discharge observations after those of Humphrey and Abbott were those taken for the information and under the direction of the Mississippi River Commission. This Commission, whose members are appointed by the President, was created under an Act of Congress in 1879, and to it was assigned the task of correcting some of the bad habits of the Mississippi river, such as wandering away from home "while on a tear;" and also of improving it as a water-way. Two parties for the purpose of taking discharge and sediment observations, and obtaining other data, were at once put in the field, one at Lake Providence, La., and the other at Fulton, Tenn., about two miles below the historic Port Pillow. The writer was detailed for duty with the latter party as assistant to W. H. Powless, Assistant Engineer, and placed in charge of the discharge observations. We began operations in the latter part of November, 1879, as soon as our instruments arrived in camp, and the work was continued until October, 1880. I was connected with the party until March of the latter year.

The methods pursued were practically the same as those of Humphrey and Abbott, departure being made only to introduce specially designed floats, and the plan of locating the paths of the floats was extended for the sake of accuracy. In a stretch of the river one mile below Fort Pillow, where for about a mile and a half the river flows through a comparatively straight course, with almost uniform section of trapezoidal form, our observation points were established. The river here is about 2500 feet wide, thirty feet deep at low stage, and sixty-two feet at high stage. By reference to Fig. 3, the position of the observation points will be understood. The sections through K, D, C and W were parallel and one hundred feet apart, and so located as to be as nearly as possible at right angles to current of the river. The velocity floats were put into the water at section K, and their points of passage over the other sections, as at F, L and T were located, and their times of passage from one to the other of these sections were noted. P and N were range signals for use in connection with putting out floats on section K, and R1, R2, R3, etc., were range signals to determine positions K1, K2, K3, etc., so that the floats could be put out from the boat at equal intervals across the stream. The signals M and N were used to "range in" for taking soundings on sections D, C and W. Section C was the one through which the discharge was measured. Its general form is shown in Fig. 4. The locating of all floats was done at E, which point was exactly 1000 feet from the discharge section measured at right angles thereto. This arrangement gave convenient base lines of 900, 1000 and 1100 feet respectively to sections D, C and W, and all angles to floats were read to the right of this base line. All points such as F, L and T were plotted upon the maps by using a table of tangents, all being measured from the base line through E. A longer base line was desirable, so that the angles to points near the farther shore would not exceed 45 degrees, but the shape of the shore line would not permit. However, this was a great improvement over the plan pursued in the Humphrey and Abbott surveys, as in the latter the base line was limited between the three lower sections; or, for observation on the discharge section, it was but 100 feet, and to the others it was 200 feet, a length very small in comparison with the width of the river, which was in their observations probably 3000 feet. Besides at E, observers with transits were stationed at D, C and W. The vertical cross hairs of the instruments at the latter points were set accurately on their respective sections, and were only used to determine the passage of the floats over the sections. The four observing points were connected by a telegraph line, and each point equipped with a telegraph key and sounder. The observer at W used two stop watches, one to time the floats from section D to section C and the other from section D to section W. These independent times acted as a check one upon the other and gave the time of passage of floats over their paths as shown by observed angles. Each observation point was protected by a rude shed five feet square, open on the river side only. The usual transit tripods were discarded, and instead of each, a heavy post was sunk well in the ground, to which the transit head could be clamped easily and quickly. Their vertical axes were thus at once "brought over the tack" without the aid of a plumb bob or the usual profanity. Once secured, the vertical cross hair of each was clamped upon the permanent signals opposite. These signals M were diamond-shaped and with their quadrants painted as shown in Fig. 5. Each instrument when once fixed upon its target opposite was not moved during an entire set of observations. This was also an improvement, as in the H. and A. surveys the instrument corresponding to D was first used for line as a float would pass its section, and then to take angles upon the float's passage over sections C and W. W was used in the reverse operation. It is not necessary that we should have been on hand to see W go through his various evolutions when the floats would be travelling over the 200 feet of observing ground in from twenty to thirty seconds, to understand that it was an accident when he succeeded in doing everything accurately. He must upon the start of a float get it within the field of his instrument, then "cut it" with his cross hair, and at a signal from D clamp and read his vernier, either record or remember the degree and minute of the angle, unclamp his instrument and hunt up the float, and repeat the operation for Section C, treasure up another angle, and be all ready to clamp his cross hair on his own section, hunt up the float to give line to D for the latter's angular observation, and by all means not forget to give the signal of its passage. In the case of a float fifteen hundred feet out from the observing station it would pass through eleven degrees of the field of his observation, and a mistake of one minute (quite easily made) in reading the angle on the section C would make an error in the location of the float on that section of about seven feet. If, in observing the same float, an error of one minute be made on sections D and W and of an opposite nature to the error on C. the path of the float when plotted might be decidedly crooked, Then again if W was not agile enough to get his instrument on his section in time to give the proper range for the final angle, the latter may have plotted "clear off the sheet." As a comparison; by our system, the same float would have been ranged in accurately at each section; it would have crossed but eight degrees of the field of observation at E and an error in observing the angle on the farthest section W, of one minute, would have made a difference of less than two feet in the distance from shore. It might be well to mention in this connection that the floats were located not only to get the position corresponding to their velocity, but also the actual path described by it, which was necessary to make the proper reduction in figuring out its velocity.

The double float system, as in the H. and A. surveys, was used, except that the floats were of special design for our work. To whom belongs the honor of the design I do not know. Both upper and lower floats were of tin. They are shown in plan and section in Fig. 6. A is an air chamber in each. When in use the wire or cord that connected them was attached at B. We used No. 25 copper wire at first, but it would kink and break so frequently and thus cause the loss of floats that it became necessary to use silken cord of slightly larger diameter. C is a ring of lead attached to and forming a part of the lower edge of the sub-float. This float would sink rapidly while the air chamber at the top and the leaden ring at the bottom stiffened it and also caused its axis to be vertical at all times while in suspension, and the cord being attached at its centre had no effect in tipping it out of this position. The two cross-wires also added stiffness. The form of the surface float offered very little resistance to its movement through the water, while none of it was exposed to the wind except the small flag inserted in its centre to aid in sighting it. Its air chamber was of sufficient size to more than counterbalance the difference in weight and buoyancy of the lower float. The dimensions on the figure may not be accurate. They are as I recollect them.

With the foregoing minute detail it is scarcely necessary for me to follow through an entire process of observing a float. Each float as observed was given a number and in order beginning at the shore line. The observer at E recorded the angles; the one at W the times, and the recorder in the boat made note of the soundings and relative force and direction of the wind, all under corresponding numbers which were afterwards combined in one set of notes. The reading of the gauge was taken before and after each set of observations and, when necessary, between times. The boat, after a float was started, followed closely and the leadsman took the sounding as he crossed the line of range boards of each section. The floats were run as nearly as possible at mid depth of the discharge section. Signals of the hand or voice were not used. Upon the approach of the float to any one of the sections, the observer at that section gave a notice of warning to the others by two clicks on his key and one click as it passed his wire. When an observation failed for any reason, a proper signal notified all that it was to be repeated. When the current was slow but one skiff party was necessary, but when it was running swift, two boats were used, working one from each shore until they met.

The resulting discharge cross-section of each set of observations was plotted to scale, the position of the various mean velocities as deduced were also plotted in their proper place of that section. The section was then sub-divided, and each portion was given its proper mean velocity, all reduced to seconds of time. The products of these various parts into their mean velocity were combined to get the total discharge, and the mean velocity of the section was obtained by dividing the discharge by the total area.

From the published records of that survey I find that the maximum mid-depth velocity as recorded by a single observation was 10.77 feet per second. The greatest mean velocity was 7.91 feet per second, the minimum 2.13 per second. The greatest discharge as calculated was 1,117,425 cubic feet per second; the smallest 154,198 cubic feet.

One set of discharge observations was taken each week-day, weather permitting. In connection with these the slope of river surface was taken once a week. The elevation of the water at the shore line was noted at intervals of 1000 feet and covering a distance up and down the river from the discharge section for a total distance of 10,000 feet. For this purpose oak stakes, as shown in Fig. 7, were driven in the water at the shore line until the top of the hook A was at the surface, and the heights of these various stake hooks were found by taking the difference of level of each with a corresponding permanent bench mark in its locality, the relative heights of the latter having been carefully determined by repeated sets of levels when established. All the stakes were driven at the same time, two adjoining stakes being driven by one person, the first of each at a given signal, and the alternate ones as soon thereafter as possible, the interval being about one and a half minutes. The average slope per foot over the entire distance varied in 38 sets of observations, from .00001444 feet when gauge was 4.7 feet and river falling, to .00007397 feet, gauge 31.9 feet and falling. In these observations, the average fall over the entire reach was not, with a few exceptions, any indication of the fall at the discharge section, the difference usually being as much as 20 per cent., sometimes greater and sometimes less. The general results were unsatisfactory, and gave clear proof that slope observations at the shore line, influenced as they were by so many local conditions, were wholly unreliable. There should be a uniformity in the slope at mid-stream of such rivers if the cross-sections were to remain constant or nearly so, but it cannot be taken with accuracy there. The river in the vicinity of our work was undoubtedly as near fulfilling a condition of uniform cross-section over an extended distance as can be found in large natural streams.

I note that in applying the H. and A. formula to the various sets of observations that it gives results for mean velocities differing widely from the observed. Also that the value of C in the general formula, ($v = C\sqrt{rs}$,) must stretch from 85 to 156. Its various values happen without any regularity except that where the observed slope seemed to be too small, C was correspondingly large.

From the tabulated results of velocity and discharge at this point of the river we find that a mean velocity of three feet per second meant a discharge of about 250,000 cubic feet per second, and a 7.6 feet velocity was good for a discharge of about 1,040,000 cubic feet per second. A difference of one-tenth of a foot per second of mean velocity in the first case would mean a difference of 25,000 cubic feet per second in the discharge, and in the second case, a difference of 104,000 cubic feet, amounts entirely too large for a fair approximation. In using the Kutter formula to find the mean velocity from the slope it would require but a difference of .0009 feet in a distance of one thousand feet, to change the smaller velocity enough to effect the corresponding difference in discharge, and it would require but .003 feet in one thousand feet to bring about the great difference in the last case. When it is considered that it is impossible to observe any system of gauge readings to the fineness indicated by the first difference in slope, and that the local conditions of the water surface at a gauge may be effected by even more than the latter difference, it is apparent that much uncertainty is attached to a mean velocity computed from slope reading.

Then again, should we be satisfied with our slope observations we find that the value of N must be carefully considered. Among the schedule of values assigned to this co-efficient by Kutter, it would appear that .025 would be proper for the cases under discussion; as every indication pointed to fairly regular and sandy bottom, except near the Tenessee shore, where there was a smooth shale. Using this value in a case already cited where the average slope was .00001444 and with the corresponding mean radius, the value of v obtained by the formula is 3.076 feet per second. The observed mean velocity of this same case was 2.20 feet per second, showing that something was decidedly wrong. If we alter N by .00125, an amount too small to place it within the range of the next quantity in the schedule either higher or lower (for they vary by the quantity .005) we find that we get a value for *v* differing by one-tenth of a foot. This small change in value alone would cause the large difference in discharge already referred to, namely 25,000 cubic feet per second, and indicates what great differences can be made in the calculation of discharge by the Kutter formula by assuming slightly different values for N. To make the calculated velocity equal to the observed we find that N must be assigned a value of .0384. We find that its value must range from .022 to .040 to satisfy the various observed results. Of course the bottom did not change in all these times, consequently the assumption of a value for N for any of these observations is proven worthless, or else these various observations were defective, either as regards the slope or the mean velocity. We strongly suspect the slope, for the reason that there is a great regularity of change of velocity and consequent discharge when compared with the gauge readings and mean depths of the river in the tabulated results of the year's work. We insist that the slope observations were taken with all possible accuracy, and that if they be unreliable, then the general plan of calculating mean velocity from slope observations is unreliable.

If then, results based on slope observations and assumed values of N are not to be depended upon, what can be done to determine discharge in such cases as these? It is our opinion that the best results will be obtained from actual observations of velocity. The absolutely correct results would be obtained from a correct cross-section and the mean velocity of all the filaments of water passing through the same. A very close approximation of the first can be easily obtained by soundings, and must be had for any method for calculating the discharge. It would be highly impracticable to observe the velocity of every filament, but with that plan as a standard of absolute correctness, it can certainly be approached; how near, is to a large extent dependent upon the observer. The general rules should be (1), to establish by repeated observations, carried on under various conditions of wind and gauge-readings throughout the work, the relation between the mean and mid-depth velocities at the section under observation or, better yet, the proportional depth at which the mean velocities throughout the section are located; (2), to observe mid-depth velocities or those at the positions where the mean velocity is located, and to take enough observations crosswise of the channel to catch every change of velocity and also to eliminate as far as possible, errors of observation. With this data well established the calculation is simple. Each mean velocity in the various parts of the section is to be multiplied into its corresponding area, and the aggregate of these results will be the discharge. We note that the Burlington experiment already referred to determined the position of mean velocity for that particular work at 0.622, or $\frac{5}{8}$ of the depth.

As to the particular system to be used, we believe the double float system, carefully conducted, will give good results. Current meters, carefully rated for coefficients, and used with chronograph, will give very accurate results. The former depends on the average of its approach to, and departure from, the discharge section for its mean velocity through the same, while the latter gives the condition of things immediately at the section, as it is held stationary there. The meter cannot be used at great depths—say of more than twenty feet—nor for high velocities, as in swift currents it would be difficult to hold in place the craft from which the observations must be taken and the meter itself. In the experiments at Burlington comparisons of these systems were made, showing very slight differences, so slight as to practically give the same results. The current meters recorded the actual pulsations of the water, thus representing every slight change in the velocity. All these oscillations within the path of a moving float are averaged.

The desired accuracy of observation of the discharge of a large stream would naturally govern the expense and time bestowed thereon. Calculations based upon gauge readings and a few well-distributed surface-floats at a cross-section, the contour of whose bottom is known, would cost but little, and may be sufficiently accurate for the purpose intended. Gauge readings alone, used in connection with the tabulated results of velocity observations previously taken at the same point, may answer the purpose. The slope formula method, with a most accurately observed slope, can only be used to a good advantage after an extended series of velocity observations have been made to determine the value of C in the general formula, and therefore is but a slight improvement upon the gauge-reading method. In fact, it seems necessary, in order to get fair results by any system, that carefully-conducted velocity observations must be made to establish constants or coefficients for the more primitive methods. To any who would undertake them we would say, if is must be done, it is worth doing well.

In the preparation of this paper I have quoted historical information from the book of Humphrey and Abbott, and have referred to the published reports of the Mississippi River Commission upon our particular work, for some data.

DISCUSSION.

PRESIDENT HUNT: I could not help but think as Mr. Davison read his paper that if I had only had the data in his paper as a precedent in connection with the work that I had in the United States Coast Survey, in 1871 and 1872, and have had some convenient rule for the use of "varying constants" like C and N, it would have saved me many a weary mile of walk in checking discordant results.

The paper is certainly one that is of interest to us and I know there are a great many of you who will at least ask questions about it; I would like to state one further fact about it, and it is of interest in carrying out the same idea that Mr. Davison has spoken of as to the difference in velocity of streams due to different wind currents, the fact has been found that varying wind currents over the Niagara River change the depth of that very rapidly moving stream, just above the falls for a height of two feet. When there is a strong wind blowing toward the Canada shore it blows the bulk of the water towards that bank to the extent that the depth of the stream is lowered to the amount of two feet near the American bank. That was quite an important matter to be taken into consideration of the water supply for the new tunnel system and of the present canal.

I would like to ask Mr. Davison with reference to the Pitot tubes to which he refers in his paper; have these ever been used in determining discharge volumes of rivers successfully? They work very satisfactorily with gas currents; that I can vouch for from my own observations.

MR. DAVISON: They only give fair results in hydraulics.

MR. HUNT: In cases when you have a difference in specific gravity, or of water varying in degrees of mud and other emulsions in water, is any difference made as to the condition of the actual specific gravity or the consistency of the water in making the calculations?

MR. DAVISON: In any case where the tube would be used to detect the small differences between the varying velocities in a vertical section, the slight variations in the water surface in the tube would be very hard to observe. I never saw it used, and only know of it in theory. If used at all, the effect of varying densities of the water would not be indicated in a Pitot tube unless a lighter fluid than water were used in the tube, and then only in the case of the water being at rest. In flowing water the effect of varying density would be an indivisible part with the effect of the *vis viva* of the flowing water.

MR. HUNT: That was not exactly the question; it was whether, in your ordinary calculations, the consistency of the water was one of the constants?

MR. DAVISON: No, sir. In connection with this same survey we took sediment observations besides the discharge observations, a matter which I did not touch on in this paper. We did not take these sediment observations in order to determine the difference in velocity of varying degrees of sediment-carrying water, but to show the relative amount carried by the river as a whole, and also at the various depths. The sediment was taken at three points in the depth, at the surface, mid-depth, and very close to the bottom at a number of points across the stream. They were taken in two sets. All of one set were combined to give a general result of the amount carried by the river, and then they were combined in relation to the surface, mid-depth, and bottom.

MR. W. G. WILKINS: I notice in the last edition of Kutter's work, translated by Hering and Trautwine, the translators say in their preface that it seems very doubtful whether any general formula can be made applicable to very large streams, and state their opinion that the Kutter formula is not intended to apply in all cases, as it is difficult to obtain the slope accurately. Mr. Davison shows very clearly by the results of his experience that it will not. He also shows the difficulty of determining the slope along the shore. Now what is the slope to be used in the formula; is it the shore slope, the slope at the middle, or the mean slope? If it is such a difficult problem to obtain the slope along the shore, it would be very much more difficult to obtain the mean slope, and the translators of Kutter's work seem to recognize this difficulty.

Prof. J. W. Langley: There are some phenomena observable in very large streams that I suppose belong more or less to all streams that escape our observations for the reason of the conditions necessary to render them visible not being ordinarily obtained. It was my fortune to live for a year and a half on the Mississippi river, about 3 miles above the bar, being stationed there in the navy during the late war. There is a very peculiar phenomenon there, which is rendered visible from the fact that the true bottom of the river, the bottom of the fresh water of the river, is formed of the brackish water backing in from the Gulf. Now one would suppose, under these circumstances, that the flow of the stream would be smooth. On the contrary, it is exceedingly turbulent. The whole surface of the stream is continually boiling like a pot. I have seen almost as a normal phenomenon varying masses of mud, boiling mud, of the diameter of this room, coming up with a rush and a roar. It is almost certain death to fall overboard from the ship. I happened to be a very unwelcome observer of an accident of that kind in which two men fell overboard from a little boat. They only fell about a foot and a half to the surface of the water, and they disappeared like stones. They never came up again.

This boiling mass is rendered visible because the salt water from below is brought up by eddies, and, mingling with the mud, renders the whole mass visible, so that every movement of every square inch of water on the surface is perfectly visible, filament by filament,—not in masses, as in ordinary clear water or homogeneously muddy water. It certainly is hardly too much to say that a very large element of motion of the water there was vertical in its action as well as horizontal. I think I can see one reason why this poor "V," which our orator has spoken of, should have had such hard treatment. It is evident, I think, that any line of current meters stretched across a river of that kind, where you have currents in every conceivable direction, would not give a reliable average unless those meters were strung at very narrow intervals and across the entire width of the river. Now at that point the river is at least 60 feet deep. There is nothing there apparently to disturb it. The bottom of the river is supposed to be smooth, the banks are of soft mud, almost as fluid as the water itself, and yet that ebullition is so great that logs, if they have projections to catch the water, are made to turn somersaults, like the spokes of a wheel, in going down the river. This phenomenon, this boiling, I may almost say, is an intermolecular strain. And this intestinal motion explains to me what Mr. Davison has so ably brought out in this paper,—the extreme difficulty, not to say the impossibility, of finding any formula which will apply to all rivers, and particularly to large rivers.

Mr. Thos. P. Roberts: Mr. Davison has brought up a subject, or rather a puzzle, which hydraulic engineers have so far been unable to solve in a very satisfactory manner. Whilst I have not, in recent years, given the gauging of rivers much attention, I have, nevertheless, failed to observe that any material improvements in methods have been made since Mr. Davison's experience on the Mississippi. He has clearly stated the difficulties that exist in devising formula applicable to every case. I mean by this that in order to determine the annual discharge of a great river with any reasonable degree of accuracy, the observers must be kept constantly stationed on the ground, and record, from day to day, two things: 1st, the stage or depth of water: 2d, its velocity by the prescribed methods. The most troublesome factor which disturbs the relationship between depth and velocity is referred to by Mr. Davison as the slope, and the slope may differ very materially with the same recorded depths. It varies for the same depth as between a rising and a falling river, and again it varies in these respects with the *rate* of the rise or fall of the surface. In a very rapidly rising river the slope is, for the time being, abnormally increased, and with it the mean velocity and, consequently, the discharge. Several years ago, in a sudden flood in the Allegheny river, which reached a height of 27 feet, for several hours the velocity of the surface in mid-stream was nearly 11 miles an hour, while upon the occasion of the unprecedented flood of 1884, the highest velocity I observed in watching the passage of drift between the same fixed points was not quite 8 miles an hour, so that the actual discharge for a certain period was fully as great with the 27-foot as it was with the 34-foot flood. Hence, I agree quite fully with Mr. Davison's conclusions, viz., that without the constant maintenance of observers at selected stations, we will be unable to obtain for instance the annual discharge of any great river; and as this is expensive, we must content ourselves with approximate figures based upon the daily gauge readings and average discharge for those readings as given by previous experience. If too much is "allowed" for one day, the next, with a less slope, may balance the account. This is somewhat the method sometimes pursued by one who sets his watch by noting the average time of a number of clocks in a jeweler's store. About as many clocks may be fast as slow, and the average may be nearly correct, and then again it may not be nearly correct.

In regard to the variations of the slope as shown between the long stations,—10,000 feet above and 10,000 feet below the measuring section,—and the slope as actually indicated within a few hundred feet above and below the same measuring section, it occurs to me that the most accurate results should be with the slope factor taken on the shortest range. As I understand it, there were wide and comparatively shoal parts of the river a mile or two distant both above and below the measuring section. We might refer to the measuring point as about the middle of a pool between shoals. In cases of this kind I have observed upon the Ohio, the Missouri, and the Monongahela rivers, and I presume it is the same on the Mississippi, viz., that on the shoals or "rapids" the surface does not rise so fast as it does in the middle of the pools. The waters appear to "bag up" to a certain extent in the pools; hence the true slope at the measuring point may not be given by a plane whose termini are so far away above and below. On the Missouri, above the great falls, I observed a point in a comparatively wide and straight reach of the river where the average of the fall per mile was say 2 feet per mile; where in one pool of several miles length, practically level at low water, the rise at its head and foot was about 6 feet, whereas in the middle of said pool the rise was nearly, if not fully, 10 feet with the same flood. From the middle of this pool to its head, the surface must have been nearly a level, whereas down-stream it presented a decided slope.

Mr. Davison, referring to the vertical curve, says: "Just why the surface velocity is less than at some point beneath the surface has never been satisfactorily explained. Regarding this point I have to suggest that the explanation may be found in the study of waves. We may assume that a fluid dense as compared with air, such as water, in moving down a wide deep channel is very sensibly retarded by bottom friction, nevertheless the tendency of free fluid bodies in motion is to develop waves. Every inequality upon the bottom tends farther to disturb the filaments of water and cause them to undulate more or less, but the effort to produce corresponding waves or undulations upon the surface is interfered with by gravity and the wave force or energy is dissipated in swirls, etc.; the dissipation of this energy at the surface is in every direction, therefore some of it directly against the descending sheet. This effect I argue must be greatest immediately at the surface. I have seen it somewhere stated that anything which prevents the passage or clearance of the lateral or following wave properly due to a given sized vessel at a certain speed, interferes with the velocity of the vessel. At all events waves are not usually propagated upon the surface of deep rivers, and whatever it is that prevents their formation is probably the cause of the development of the parabolic curve noted in the vertical sections of the river by Humphreys and Abbot in their *Physics and Hydraulics of the Mississippi.*

I would like to add in connection with Prof. Langley's remarks regarding the agitation of those waters in the Mississippi that I observed very much the same thing at the junction of the Madison river with the Gallatin, which form two of the forks of the Missouri, the third being the Jefferson. They came together nearly at right angles, and there was a violent commotion there and yet there was a perfectly smooth and sandy bottom. It seemed to be very much deeper at that point.

Something of the same sort can be seen along the edge of the Gulf Stream under certain conditions of the water. It seems to be very much swirling and broken, and catches and retains the gulf weed and other drift, everything moving violently around in circles. It may be caused, perhaps, by the difference in gravity of the warm and cooler waters adjoining, and I would also suggest the idea that the difference of gravity between salt and fresh water might occasion this in the case of streams reaching the sea.

MR. MAX BECKER: This paper of Mr. Davison's seems to confirm two points particularly. One of them is to the effect that the most rapid current is somewhere below the surface; and the other one is that the hydraulic formulæ seem not to be in very perfect harmony with each other; that they are rather deceptive and generally unreliable. I fully agree with that latter point, and I will say further that the longer the formulæ the more deceptive they generally are.

As to the first point, that the current is strongest a little under the surface, I can tell you a little personal experience of my own which I think will go far to prove this fact. When I was a boy I used to go swimming occasionally, and one day, having gone into the river and paddled around a little while something seemed to be the matter with me. I could not keep up on the surface. I went down beneath the surface, and the fellow that caught me by the hair and pulled me out, after I had recovered my senses, told me that after I had disappeared below the surface I was "going very fast." In fact I was very nearly gone altogether.

Now I would like to ask Mr. Davison whether there were any measurements made at the time when he was with the River Commission that would go to show the effect which a reverse slope in the bottom of the river would have upon the curve representing the current line. In other words, supposing the bottom of the river to be rising in the downward direction of the stream, what effect would that have on your vertical curve? I imagine there would certainly be some movement noticeable in the abscissa representing the strongest current line. If the bottom of the river should rise to the surface of the water, there would be a pool without current and the curve representing the flow at different depths would become a straight line from the top to the bottom.

If you then take a slice off the crest of the dam and create a weir the flow would be near the top, and a slight current-curve would appear near the surface. If you continue shaving off the top, and by degrees make the bottom slope of the river more parallel to the surface slope you will gradually approach this curve of Mr. Davison's. Some effect is certainly produced by a convergence or divergence of the bottom and surface slopes upon the shape of the current-curve, the extent of which it would be interesting to know. Have your observations developed any facts which would furnish any data for determining a fixed law on this point? Have I made myself understood?

MR. DAVISON: I understand the question fully. There was nothing done to determine the relation between the curve that might occur at such a point as he asks about. We were stationed in a point of the river where a certain distance above and below there was less water than where we were. At the sections where there was less depth the water would assume for itself a greater mean velocity if the width remained the same as in the deeper place. This mean velocity should be an ordinate of a parabolic curve of a new parameter, and, as Mr. Becker hints, its axis, or the maximum velocity, would undoubtedly be forced nearer to the surface.

MR. WILKINS: There were some observations made on the river which showed the bottom was the source of waves. I do not know that there is any law showing any relation between the velocity and the height of the waves. The investigation did not go that far.

MR. WYNN: It seems to me that would depend on the condition of the water, whether the river was rising or falling. If it was rising the point of maximum velocity would be nearer the surface than when falling. All these observations seem to show that the current is moving in a straight line. It is not the case in a large river. The current is rapid and from one side to the other there are counter-currents at different depths, probably running across each other in different forms. Steamboats in passing from the Ohio to the Mississippi, particularly if the latter is a little high, have had the water come up in waves over the top of the capstan. I have been on boats where if it were not for the bulkheads the water would have put the fire out. The theory was that we were running into counter-currents.

I have often noticed this boiling in water as it struck a bar. It would whirl up, and revolve, but I think it would not be possible to measure the current at different points so as to be accurate, yet I do not think it would take so many years to do it. All that it would be necessary to do would be to take the water at different stages and get the aggregate.

With regard to the amount of sediment carried in the water I think there is a very large percentage that is not in suspension in the water, that is moving along the bottom, moving slowly, forming bars, cutting off the top, floating away in other ways. I have a piece of stone taken from the Mississippi river, from the western pier of the St. Louis bridge, polished smooth as it can be. That was 105 feet below the surface, and at the time they were making the excavations there were 60 feet of gravel on it. It seems to me that the whole body of that mass was moving at different times as well as the water. Then the flow of water through the river I think does not measure exactly the actual amount going through. Subterranean currents carry some.

Mr. Swensson: Allow me to ask Mr. Davison whether at the place of observation he speaks of there were any counter-currents or terrible ebullitions in the water or whether it was a comparatively quiet spot.

Mr. Davison: Where the discharge observations took place it was a mile and a half below where there were noticeable swirls. The water at that point up to a stage of 10 or 15 feet seemed to be very calm. It was only when the river was very high there was any disturbance on the surface.

MR. SWENSSON: If that is the case, the effect of such disturbances was largely eliminated. If eliminated, should we not confine our discussion to the formula and the quantities composing same, as well as the means of obtaining them? Could these quantities be determined with exactness, I suppose the formula would be correct. Of course, I presume that an intelligent engineer, when undertaking to determine the velocity of a river, will select a spot in that river, where swirls and counter-currents do not affect his work and, consequently, need not be considered. Other conditions, existing in comparatively calm rivers, affecting the velocity, are difficult enough to determine. Instead of bunching these conditions together under one head and calling it a co-efficient, should we not endeavor to ascertain what causes the variations in this co-efficient and substitute a mathematical expression for some in which the uncertainty is reduced to a minimum? For example, could not that "N" in the formula be somewhat closer determined by studying out the bearing that the roughness of river bottom and relation of depth to width of river, has on this quantity.

These conditions, as well as others, were referred to in Mr. Davison's paper as being variable and materially affecting the actual velocity, and should be worthy of our careful consideration.

MR. DAVISON: O. Kutter made up a schedule of values for that quantity which ran from the minimum to the maximum, as shown in the paper. Now a bottom similar to what the Mississippi was at the point where we were taking our observations should have been .025, but, as I showed in one instance there, which was only a sample, that value had to be raised to .0384 in order to make the velocity figure out what it should.

MR. SWENSSON: What did Kutter base his table on?

MR. DAVISON: He based his table on experiments in open streams and in pipes, but I do not consider it applicable to large streams.

MR. ROBERTS: I think I can explain or suggest an explanation for one error. They discovered one difficulty; that was in taking the slope or surface slope referred to in the paper: Taking it at points 10,000 feet above and below the measuring section. As I understand it, the water was shallower at those points than at mid-section. My observation on frequent occasions on a number of rivers has been that the water rises faster in pools, deep places, than it does in shallow places. I have known on the upper Missouri 11 feet at one place and 5 feet above and below and yet there was a down stream slope all the time. So when these observers would take across 20,000 feet and use that for an average slope they would certainly get erroneous results.

Mr. Kauffman: I cannot see where the use of making such observations on large streams comes in. I do not see the advantage of finding out how much silt the Mississippi was carrying any more than I would try to find out how much sand was deposited on the beach by the ocean. I do not see what good it would do you. If there is any use for it I would like to be enlightened.

Mr. Davison: The several points that they had meant to determine by these observations were to know how much water was passing at various stages of the stream and the corresponding velocities, in order that they might be perfectly familiar with the conditions should they take hold of a section of the river to improve it, and confine it within artificial banks, and force it to assume a new cross-section without overflowing its new confines.

Mr. Kauffman: I confess I cannot see the matter yet. I do not see what they intended to get at by any observations of that kind.

Mr. Davison: I would say to Mr. Kauffman that on the *excellent* results arrived at at, Fulton, the Commission has spent several million dollars, and in order to spend those millions judiciously they had to have the results of our work.

Mr. Wilkins: One fact that was brought out in those surveys there was this, that on a rising stage the bottom of the river in this " Fulton reach " as it was called, was higher than on a falling stage indicating a deposit on the rising, and scour on falling stage and that the maximum difference in elevation of the bottom of the river was over 5 feet.

Mr. Scaife: I would like to suggest one idea that might be a reason for such investigations in connection with others, that is, in the prediction of floods. Every year, as the country increases in population, and as river banks get settled, the rise of each foot above the normal level of a stream causes increasing loss of property, and sometimes loss of life; so that we find in old countries, like France, for instance, the régime of each large stream is watched very closely. There the government engineers keep accurate records of the rainfalls, river depths, etc., and predict floods for some time in advance. Knowing the rainfall and the time required to affect the different streams, they can predict very closely what height of water will be produced at different points. In view of the vast injury done by floods in this section of the country and elsewhere, it seems to me that our government engineers might render very valuable services in this direction, by acting in conjunction with the weather bureau. In this way, every considerable rise of streams might be known at important points, some time in advance, thus enabling interested persons to prepare for the same. River navigation would likewise be greatly benefited by such information.

MR. DAVISON: I would add that three or four miles above our section the river was probably in one of its widest portions, over two miles wide, and a few miles below us again the river was contracted to about its narrowest channel between Cairo and New Orleans. Our observations while they were for general use, were also in connection with the question of improving this very wide portion of the river. Any of our Pittsburg and New Orleans steamboat men will know all about Bullerton tow-head, the wide place to which I refer. The river being so very wide, naturally there was very little water there when the river was low, and that is one of the principal points where the government has been engaged in narrowing the stream to make it navigable at all stages.

Adjourned.

R. N. CLARK,
Secretary.

MEETING OF THE CHEMICAL SECTION OF THE ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

APRIL 21ST, 1892.

Twenty-six chemists present. Temporary chairman, Mr. John W. Langley presided.

Minutes of the last meeting were read by the acting secretary

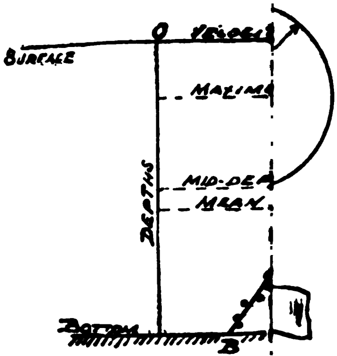
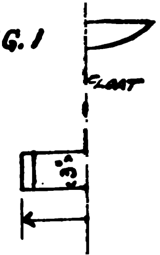


FIG. 1



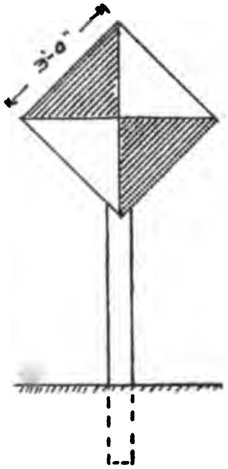
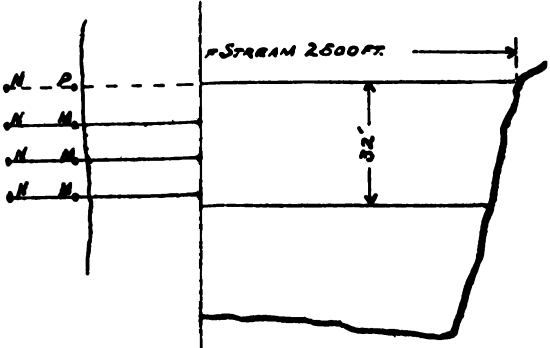


FIG. 5



IG. 4

Mr. J. C. Barrett made the following report of work done for Mr. Camp, on Handy's method for phosphorus determination:

By a large number of analyses he had proven the exactness of the method when applied to pig-iron and to steel of low carbon content. He had failed to obtain satisfactory results by the method when working on spiegeleisen and ferromanganese.

Mr. Philo Kemery said that on account of poor gas supply he had been unable to make an investigation of Mr. Handy's method; he however hoped soon to make examination and to report. The method proves to have been of general interest, and is being tried in the laboratories of W. Dewees Wood Co. and the Wayne Iron Works.

It has been in successful and continuous use in the laboratories of the Pittsburg Testing Laboratory, Carbon Iron Co. and Pittsburg Steel Casting Co. for more than two months. It has been found to give accurate results more uniformly with ordinary working than the gravimetric method formerly used.

The process has been examined and adopted in the laboratory of the Eliza Furnaces (Laughlin & Co.).

The reports of the members of the Committee on Investigation of the Phosphorus Method were discussed as follows:

DR. CHAS. B. DUDLEY to Mr. Barrett: Will you be kind enough to describe the process which you used in your investigations?

MR. BARRETT: In the one case I used Drown's latest method, involving the oxidation of the phosphorus by $KMnO_4$ solution, while in 1.13 nitric solution.

In the method designated as "gravimetric" I dissolved the metal in nitric acid and oxidized carbon and phosphorus by evaporation to hard dryness.

DR. DUDLEY: What reducing agent did you use in the first process?

MR. BARRETT: I used ferrous sulphate, finding that it was less violent in its action than sugar.

DR. DUDLEY: You obtained lower results in ferromanganese analysis by Mr. Handy's method than by the second method. Do you mean that less phosphomolybdate, by weight, actually separated out when that process was used, or that there was an error in the volumetric estimation of that which did separate?

Mr. Barrett: I think that less phosphomolybdate came down when Drown's process was used. I used a large quantity of potassium permanganate for oxidation and boiled the solution for a long time. I do not think that the carbon of the ferromanganese had been completely oxidized, for the solution, after clarifying by means of ferrous sulphate, was still brownish. Probably the phosphorus was not completely oxidized either. By the Drown-Jones method, more than a year ago we failed to get all the phosphorus in spiegels and ferromanganese. I believe that Mr. E. F. Wood has had similar difficulty with his chromic acid method when oxidizing the carbon of ferromanganese.

Dr. Dudley: I wonder if any of you have had the trouble which we have had in the use of phenolphtalein indicator. It is well known that this indicator does not give good results when ammonia is present. We found that in titrating high percentages of yellow phosphomolybdate, *e.g.*, from three to seven-tenths per cent. that the color of the indicator faded slowly as the titration proceeded, and the end reaction was not very sharp. I suppose this is the reason why Mr. Handy recommends a special standardization for high phosphorus work.

Mr. Handy: I am very much obliged to these gentlemen for having so carefully investigated the phosphorus method; of course the determination of *phosphorus* in *ferromanganese* forms less than one per cent. of the phosphorus estimations daily required in our iron and steel works laboratories; yet I am sorry to see that the method seems to be poorly adapted to this small use. I am hopeful that with very slight modification its use can be extended to cover the analysis of ferromanganese, spiegels and the like; I shall experiment at once and report my results. In regard to the time of the method, I am glad to see that, without much previous practice but with better appliances than I had, a determination of phosphorus has been done in thirteen minutes.

Replying to Dr. Dudley's remarks concerning the failure of phenol-phtalein indicator in work on metals with a high percentage of phosphorus, I cannot say that I have always noticed the fading phenomenon which he speaks of, and cannot yet believe that the phenomenon is not due to some accidental impurity. I shall do futther work, and if I am wrong I shall promptly acknowledge it. Dr. Dudley speaks of the entire failure of methyl orange in the presence of the yellow precipitate. I have not tried it, but I have tried litmus, and from a few experiments I think that it may be of value in this method if phenol-phtalein is finally found to fail in the higher phosphorus work.

Chairman Langley. then called for the report of the Committee on Rules. In the absence of Mr. Jesse Jones, chairman of the committee, the report was read by Mr. Handy. It was taken up, article by article, discussed and revised. Its form was, when finally adopted, as follows:

RULES.

1. The name of our organization shall be "The Chemical Section of the Engineers' Society of Western Pennsylvania."

2. The object of the Section shall be the consideration of papers and topics connected with chemistry and metallurgy, and the encouragement of researches and investigations in the same.

3. Officers of this Society shall be a Chairman, a Vice-Chairman, a Secretary, and Board of Directors, who, after the year 1892, shall be elected at the annual meeting in January by ballot, and shall require a majority of the votes cast. Officers of the Society shall hold office for one year. Active members may ballot by mail. Ten members shall constitute a quorum for the transaction of business.

4. The Board of Directors shall consist of the Chairman, Vice-Chairman, Secretary, and two other active members.

The Section shall have Standing Committees as follows:

1. Committee on Chemical Literature shall consist of five or more members, whose duties shall be to examine the various accessible chemical and metallurgical periodicals, and report to the Section on subjects of interest found therein.

2. Committee on Methods shall consist of three members of the Section, to whom may be referred questions concerning methods of analysis and unification of processes.

ARTICLE I.

Regular meetings for the reading of papers, and discussion of scientific subjects, shall be held on the FOURTH TUESDAY OF EACH MONTH.

Special meetings may be called by the Chairman, and shall be called at the request of five members in writing.

Notices of special meetings shall be mailed to each active member at least one week in advance of said meeting.

Notices of special meetings shall contain a statement of the object for which the meeting is called, and no subject not stated in the notice shall be decided at any special meeting.

ARTICLE II.

SECTION 1. The following order of business shall be observed at the annual meeting:

1. Reading of minutes of last annual meeting.
2. Report of Secretary.
3. Annual report of the Board of Directors.
4. Reports of special committees.
5. Address of retiring Chairman.
6. Election, and announcement of election of officers.
7. Adjournment.

SEC. 2. The following shall be the order of business for the regular meetings:

1. Reading of minutes of last regular and of subsequent special meetings.
2. Report of the Board of Directors.
3. Unfinished business.
4. Reports of committees.
5. New business.
6. Miscellaneous announcements, papers, items, notes, or communications.
7. Adjournment.

SEC. 3. In all questions arising at any meeting involving parliamentary rules, Cushing's *Manual* will be accepted as authority.

ARTICLE III.

Amendments to the Rules must be proposed in writing by at least three members at one regular meeting, and adopted by a two-thirds vote at a subsequent regular meeting; providing written notice of such amendment be sent to each member with notice of meeting.

A Committee on Nomination was appointed by the chairman at the request of the members. The committee consisted of Messrs. George Faunce, F. C. Phillips and Frederick Crabtree. This committee was asked to prepare a list of officers as directed in the rules and report to the section at its next meeting, Tuesday, May 24th.

Mr. J. E. Williams, Chemist at the Pennsylvania Lead Works, then read his Paper on

EXPERIENCE WITH VON SCHULZ & LOW'S METHOD FOR LEAD ESTIMATION IN ORES.

In November last Von Schultz & Low sent the following circular letter to the various smelters and assay officers:

DENVER, COL., NOVEMBER 5TH, 1891.

We desire to call the attention of those interested to our new method for estimating lead in ores. The ordinary fire assay is frequently so erroneous and so influenced by the various impurities that are always occurring in ores, that a more correct practicable method has long been desired. We have made the matter a study and beg to present the enclosed scheme as the result of our experiments.

By the universal adoption of this method the lead assay would lose most of its uncertainty and stand on a par with copper and zinc. As smelters will not pay for lead on the basis of any wet assay, it becomes necessary to make a deduction, as in the case of copper, to arrive at the dry assay. We would suggest a reduction of three per cent. from the wet assay as being both proper and sufficient.

Our method, as detailed, is applicable to all kinds of ores, mattes, etc. With ores of special purity the scheme may be changed somewhat, if desired, with a slight saving of time or chemicals. For instance, when the silver present is insignificant in amount, no care need be taken that the reagents are free from chlorine.

Again, when antimony is known to be absent, only 5 c.c. of sulphuric acid need be used in the decomposition, and no Rochelle salt is required and only half the dilution. To ensure good work, however, we advocate the method as it stands. The time required is about the same as for copper or zinc, and the cost is less than for the fire assay, especially when the repeats and duplicates of the latter are taken into account. The sheet aluminum does not cost over $1.50 per pound, and a pound will suffice for hundreds of assays (1).

We shall be glad to have those who try our method give us their opinion as to its feasibility, and also their idea of the proper deduction.

TECHNICAL ESTIMATION OF LEAD.

Provide three wash-bottles containing the following solutions respectively: Distilled water, or, at least, chlorine-free water. Dilute sulphuric acid. One part C. P. acid to nine parts chlorine-free water. Chloride of ammonium. Make a cold saturated solution of commercial chloride of ammonium in common water. Transfer this to the wash-bottle as required and heat to boiling for use.

Treat 1 gramme of the ore in a 250 c.c. flask (2) with 10 c.c. of pure, strong nitric acid and 10 c.c. of pure, strong sulphuric acid. Heat strongly until all the nitric acid is expelled and the sulphuric acid is boiling freely. This may be done without much danger to the flask, over a small, naked frame, taking care that the flame does not touch the flask above the liquid. Cool and add 10 c.c. of the above dilute sulphuric acid. Then add 2 grammes, roughly weighed or even guessed at, of Rochelle salt. When this is dissolved add 40 c.c. of distilled water and heat to boiling. Allow to stand and settle two minutes and then filter and wash with dilute sulphuric acid. See that no sulphate of lead remains in the flask. Rinse the flask thoroughly at the faucet to remove every particle of residue and then place in it three pieces of sheet aluminum, each about one-sixteenth of an inch thick by five-eighths of an inch wide and an inch and three-quarters long. Now set the funnel and filter over the flask and dissolve the sulphate of lead on the filter (3) with the boiling hot chloride of ammonium solution, which has been heating in the meantime. Use the wash-bottle stream as a sort of stirrer until the sulphate is all dissolved, and then wash the filter well with the same solution. Keep the bulk of the filtrate as small as possible. Now heat the latter to boiling and then boil for five minutes. Remove from the heat, shake around a little to collect the lead and then fill the flask with cold water. Wash twice by decantation (4), catching the water each time in a large casserole in which any escaping particle of lead may be seen and retained. Finally invert the filled flask over the same casserole and allow the entire contents to run out. Remove the aluminum with the hand, rubbing off any adhering lead under water. Decant the water carefully and rinse the lead into a small (2½ inches) porcelain dish. Now decant as much water as possible and collect and press the lead into a thin sheet with an agate pestle (5). It is not usually practicable to collect every particle into one piece. Wash the lead several times with distilled water and then dry at a very gentle heat. The drying is facilitated and sticking to the dish prevented by washing the last time with alcohol. Brush the lead into the scale-pan and weigh. A little insoluble residue from the aluminum may frequently be seen. It may either be removed with a brush, or, from its small amount, neglected.* The lead is practically free from Ag, Au, Cu, Sb, Bi, etc. The same aluminum may be used repeatedly until too thin and light to remain at the bottom of the liquid during the precipitation. It may be cleaned with dilute nitric acid.

NOTES.

1. We have found the commercial aluminum to contain too much silicon, and are now using a much purer aluminum supplied by the Pittsburg Reduction Company.

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* See comment on (1) p. 121.

2. A small narrow griffin beaker covered with a watch-glass is more convenient than a flask. It is more easily rinsed out. The lip of the beaker allows the escape of the gas in boiling. A clean flask is subsequently used for the precipitation.

3. This method of dissolving the sulphate of lead on the filter will suffice with most ores, but where the amount of gangue is considerable, especially in the presence of much sulphate of calcium or barium, it is safer to rinse the contents of the filter back into the beaker with the hot chloride solution and heat the mixture to boiling and then filter and wash through the same filter.

4. Instead of washing in the flask, invert at once into the large casserole and wash there, removing the aluminum after the last (third) washing.

5. The lead should be in the form of a thin, compact sheet and should not oxidize a particle on drying.

As far as the direct manipulation of the assay is concerned there does not seem to be much room for improvement except in a few minor details which may not suit the convenience of every one who uses the method. A few minutes may be saved by not getting all the $Pb\,SO_4$ on the filter but dissolving what remains in the flask (after rinsing twice) and pouring it on the filter before washing the last time with the hot chloride solution. This involves the use of an extra flask to receive the $PbSO_4$ solution.

One, two or three assays may be made in the same time as the same number of fire assays, but as this "humid" method requires much more manipulation than the "fire" assay in making a large quantity, the latter would take less time. Assays by Von Schulz & Low's scheme can be done in less than forty minutes.

The first trial made in the laboratory of the Pennsylvania Lead Company was on refined lead containing over 99.98 per cent. Pb (no account was taken of the impurities, as lead assays are never reported closer than one-tenth of 1 per cent). The result on the above lead was 99.6 per cent. It was found, however, that the results had a tendency to be slightly over than under the truth, as the lead while drying oxidized a little. If this takes place to any extent the oxidation is visible and the assay can be thrown out.

With care the error should not amount to more than two-tenths of one per cent.

A number of assays were carried out, using pure lead with additions of antimony, arsenic and other impurities, but, as Von Schulz & Low state, they do not interfere.

A lump of galena was taken which contained no stains of iron or any impurities visible. This gave, by a number of tests by the Von Schulz & Low method, 86.4 per cent. Pb, and fire assay, 83.3 per cent. Pb. Another sample of a carload of galena gave 84.4 per cent. Pb, Von Schulz & Low, and 81.0 per cent. Pb fire assay. Pure galena should contain about 86.6 per cent. Pb. Hence Von Schulz & Low's suggestion to deduct 3 per cent. from the wet assay to arrive at the fire assay would seem right.

In laboratories where little lead assaying is done V. S. & L.'s method will prove valuable, as it can be carried out with apparatus always on hand. As for cost, the chemicals are all cheap. The flasks should be of the best, as breakage causes annoyance and delay. If a sand bath is used in the first operation for boiling down, there is no danger, and the evaporation is nearly as rapid as over the naked flame. In assaying matte and speiss this method has proven very valuable. It can be applied to any compound of lead that is decomposed by nitric or sulphuric acid.

Mr. Williams's paper was discussed by Dr. Dudley, Mr. Faunce, Mr. Lihme, Mr. Marsh, Mr. Manby, Mr. Handy and others.

Mr. Marsh spoke of the utter unreliability of the fire assay for lead.

Mr. Lihme said that the Von Schulz & Low's method would not work on lead silicate or minium compounds, because they would not be decomposed by nitric and sulphuric acids.

Mr. Handy suggested that lead silicate compounds might be successfully decomposed by acting on them in a platinum dish, first with nitric and hydrofluoric acids, and finally with sulphuric. After evaporation, the solution, etc., could be transferred to a beaker and the operation continued.

Mr. Manby had tried hydrofluoric acid for lead slags and had had success.

Adjourned.

MAY 17TH, 1892.

THE Society met in the parlors of the Academy of Science and Art on May 17, 1892.

Mr. Alfred E. Hunt, President, in the chair; Mr. R. N. Clark, Secretary; fifty-six members were present. The minutes of the last meeting were read and approved. The following gentlemen were elected members of the Society by ballot:

Hepburn Walker, E. Holbrook, F. S. White, Charles Q. Rawling, James J. Froenheiser, J. P. Johnston.

The Library Committee reported that twenty-nine additional volumes had been received.

From the Committee on Smoke Mr. Dempster reported that owing to the absence of some of the members but little had been done. He asked members having any information on the subject to communicate with the Secretary who in turn would refer it to the Committee.

From the Committee on Headquarters at the Columbian Exposition, Mr. Hunt reported that a meeting had been held in Chicago on the 4th of May, at which it had been reported that the Engineer Society of Western Pennsylvania would do its share in the work. A building has been secured on Michigan Avenue just facing the lake, above the Auditorium Building, very conveniently located, near the Illinois Central and Elevated Railroads, and near the lake wharf where boats will take passengers for the fair grounds. It will be a place where members of the associated engineering bodies can feel at home. Admission will be by cards, which will be given to members of the various Societies who subscribe to the fund. Secretaries will be in attendance to give information about the city and points of interest, with other information which will be of value at that time.

It will be the headquarters and a convenience for visiting engineers from all over the world. At the meeting the action of the General Committee was endorsed, and they were told to go ahead.

In view of the fact that the death of Colonel Merrill resulted in making Mr. Hunt an active member in his stead for the American Society of Civil Engineers, he asked for a colleague from this Society, and on motion Mr. Charles Davis was elected.

Mr. Alfred E. Hunt then read the following memorial in regard to the death of William P. Shinn, Esq.

MEMORIAL OF WILLIAM P. SHINN.

In the death of William Powell Shinn, our community has lost an engineer whose ability and reputation is of such a character as demands a tribute to his memory from the Engineers' Society of Western Pennsylvania, although he was not a member.

He was born at Burlington, New Jersey, on May 4, 1834, and died on May 5, 1892, at the age of fifty-eight. His was a busy life from early manhood; his first engineering work of prominence being the preparation of data for the maps of the cities of Providence, Rhode Island, and Fall River, Mass., when he was only fifteen years old.

He came to Pittsburg when only sixteen years old, and has resided here with few intervals from that time until his death.

His first work in Pittsburg was his service as rodman of the old Ohio and Pennsylvania Railroad, which has since become a part of the Pittsburg, Fort Wayne and Chicago Railroad. He served this company in many capacities, being rapidly advanced until 1873. During his service on the Fort Wayne road, he invented and inaugurated the present system of vouchers used by this road, and since become an almost universal feature of railroad and other large corporation accounting. When the Pennsylvania Company was organized, he was appointed as an expert to examine the affairs and conditions of the various lines of the company. In 1870, at the instance of the Pennsylvania Company, he made an exhaustive examination into the physical condition, the amount and value of the traffic, and of the prospects and capacity for the future development of the United Railways of New Jersey. This property not only embraced the Camden and Amboy, and Philadelphia and Trenton Railways and their various branches, but included the Delaware and Raritan, and New Jersey Canals, and the large fleet of vessels of various kinds used in transferring their traffic at Philadelphia and New York. The purpose for which this report was desired by the Pennsylvania Railroad, was to determine the advisability of leasing these properties. Upon this report the leases were made by the Pennsylvania Railroad; the outcome of which, together with his report, has shown not only the wonderful ability of Mr. Shinn in his capacity of mastering and comprehending the details of this widely varying system of properties, but also his business judgment and sagacity as well. The report also included suggestions as to the future development and management of the properties, and the thorough and comprehensive scope of the report can be appreciated by the fact that but one suggestion therein contained, that he considered important, was not specifically carried into execution substantially as suggested by him, and that this examination and report was commenced and completed within a period of three weeks. As illustrating the confidence which the management of the Pennsylvania Railroad imposed in his judgment and ability, it may be mentioned that at the time the first Interstate Commerce Bill was introduced by Senator Regan, he received a telegram from the railroad management, asking him to appear upon the following day before the Senate Committee on Commerce, and make an argument against that bill. This he did without having time to make any preparation, or having time for any consultations regarding the matter.

When the Pennsylvania Company was organized, Mr. Shinn's report, as an expert, in the examination of the affairs and conditions of the various lines to be consolidated, was the document used in the valuations which were made. In 1873 he became the managing partner of the firm of Carnegie, McCandless & Co., still holding the position of Vice-President of the Allegheny Valley Railroad; and he had charge of the building and operation of the Edgar Thomson Steel Works until October 1, 1879, developing many of the improvements and economical methods of manufacturing steel, and of blast-furnace practice, which has since caused this plant to become one of the noted metallurgical industries of the world.

In 1879 and 1880 Mr. Shinn organized the Vulcan Steel Co., of St. Louis, and rebuilt and started that works.

From 1881 to 1887 he was Vice-President of the New York Steam Co. From 1888 to 1891 he was Vice-President and General Manager of the New York and New England Railway Co., and in the latter two years President of the Norwich and New York Transportation Co.

He was one of the originators of the Mansfield Coal and Coke Co., and its President for several years.

During the past year, Mr. Shinn organized the United States Glass Co., a consolidation of sixteen manufacturing glass concerns, having a total capital of $4,000,000. He was a director in this company at the time of his death.

In 1868 he became a member of the American Society of Civil Engineers, in 1875 a member of the American Institute of Mining Engineers, in 1877 a member of the Iron and Steel Institute of Great Britain, and in 1891 became a member of the Institution of Civil Engineers of Great Britain. He was President of the American Institute of Mining Engineers in 1880, and of the American Society of Civil Engineers in 1890. His contributions to the *Transactions* of these societies were many and of great value, and are a worthy monument of his fame and ability. One of his papers before the American Society of Civil Engineers is considered by railroad men as the best ever written on the subject of railroad management, and was awarded the Norman gold medal of the American Society of Civil Engineers.

Mr. Shinn was the chairman of the Committee on Transportation and a member of the General Executive Committee of the reception of the Iron and Steel Institute of Great Britain and of the Verein Deutscher Eisenheuttenleute to America. Here he won again an international reputation for his skilful management of this undertaking, which can be best summarized by the public statement which he made in advance, as a promise at New York city, to the party upon their being intrusted to his care, that he intended to take the party—between three and four hundred people—through eighteen different States of the Union, over several thousands of miles of railroads, upon a trip of twenty-eight days' duration, and would turn the party at their destination over to the Canadian Committee at Niagara, or return them to the Grand Central Station, New York, on time and safely, without any accident. His promises in this matter were fulfilled to the letter, the party arriving in New York on return not two minutes late of schedule time, and with every item of the extended programme carried out in its fullest detail.

We Pittsburgers have, in his death, lost a citizen whose energy and ability had given him an international reputation, and many of the members of our Society have lost a sincere personal friend.

Mr. John W. Seaver then read his Paper on

IRON MILL BUILDINGS.

"To make the punishment fit the crime," Gilbert describes as the Mikado's object all sublime, and so might an engineer, when called upon to design a plant, announce as a similarly laudable ambition: "To make the building fit the mill." For it has too frequently been the practice to first settle on the size of the building for a plant and then arrange the machinery or other contents to suit. When we consider that in the majority of cases, especially those that occur in the vicinity of Pittsburg, that a mill building is merely a covering or protection from the wind and weather, we see that generally in laying out a plant the dimensions and construction of the building should be entirely subordinated to what it is to contain, and, following out this idea, all the machinery should first be located, and then the building designed as a mere cover for it. Or in case there should be cranes or shafting fastened to it, it should be framed to suit such attachments instead of shifting the cranes, shafting, or other loads around to suit the building. By following out this line of action, and planning the building last when laying out a plant, all waste room may be avoided, and the light, ventilation, and numerous other very important points best arranged to suit the purpose for which the building is desired.

When the construction of the building has been determined upon, the first question that arises should be, is the building intended for a permanent structure, or will it be removed at some future date? The reason for this question is, that when a building is only intended to serve a temporary purpose, the lightest and cheapest construction that will answer is frequently the only one that will be justified, and a construction will often be used for a temporary use that could be greatly improved upon if it were intended for a permanent structure. For instance, a light pin-connected or bolted truss, permitting of easy assembling and equally easy taking apart, would be advisable in the place of a more substantial riveted truss that would, of course, be more expensive, but probably more preferable in case it was to remain as a permanent building. But in no case should the old engineering maxim, "The construction that accomplishes the desired end in the most direct and cheapest manner is the best," be lost sight of. Nor should there be introduced in the design of such a strictly utilitarian structure as a mill building, any construction that has not a definite use and some particular function to perform. In the best designed mill buildings this feature is strictly carried out, and nothing introduced merely for the sake of appearances or because it is customary. The engineer who designs a mill building ought to be able to give a logical reason for the presence of every piece in the building; and in these days of sharp competition for contracts, a careful examination of a lot of competitive designs, submitted for the same structure by different contracting firms, will disclose very few useless features in any design; in fact, since iron mill building has grown to be the large and highly specialized business that it now has, the contractor who submits a design and has not figured it down to the very lowest notch in every respect will find that if the party who has asked him for a proposal and design has also asked other bidders to tender for it, and unless he can show a reason for the presence of every dollar's worth of material and labor which his plan represents, that he might as well have saved himself the trouble of figuring on the work. For mill building has, alas for the poor contractor, become like unto bridge building, the profits of which now are as nothing compared to those of the good old days of the sixties and seventies, when, unless a man could see at least 20 per cent. clear in a job, he would not figure on it.

The object of this paper is to give a brief description of some of the most important points in the construction of mill buildings in such a way as to enable a party who contemplates the erection of a building to decide upon what will give him the most satisfactory return for his money, and to compare the merits of the various plans and proposals that may be submitted for his consideration. This is supposing the case to be that of a mill owner sending around to builders for competitive plans and proposals, and does not apply to the case of a party who hires a consulting engineer to design his building for him and then asks for bids on these plans. There are advantages in both cases. In the first, the party to whom competing bidders submit their own plans gets all the benefit of competition in both design and price; so that, instead of taking one man's ideas, as he does when he employs a consulting engineer, he has as many engineers pitted against each other as there are competing bidders, and unless the consulting engineeer that he should employ to get up his plans has made almost as much an exclusive specialty of mill building as the competing contracting engineers do, the building, when erected from a consulting engineer's plans, will cost considerably more than when built from competing designs.

If the intending builder is really wishing a thoroughly good and substantial construction, and is willing to pay a fair price for the same instead of trying to cut the cost of the building down to the lowest possible point, and lets the competing bidders know that merit in design will have as much influence in awarding the contract as the question of price, I have very little doubt that if the invitation to submit a plan and proposal be accompanied with a clear statement of just what is desired, and a specification setting forth what unit strains, and other important points, are to be observed in the design, that the builder will secure better results by asking for competing plans and proposals from iron mill-builders in the same way that railroads ask for bids from bridge-builders, than he will by having a plan prepared by some consulting engineer, and then asking all the different builders to submit proposals on the same plan. That is, of course, supposing that the owner knows just what he wants, and is honest enough, not only to the bidders but to himself, to say what he really wants, what he will take and will pay for, and what he will not take. For, speaking of honesty on the part of an intending builder, when a man has made up his mind to put up the cheapest possible structure that will answer his purpose, how often will he say so to the parties he asks a price from? Instead of that, he almost invariably states that he wants a good building, not infrequently adding the requirements, that it must be strong and substantial, and also mentioning the fact that he wants it cheap, and completely ignoring the fact that in an iron mill building, just the same as in an engine, a piece of machinery, or a furnace, goodness, strength, and substantiality with cheapness are incompatible. For cheapness means the least possible amount of material and labor, and the cutting of everything down, from the designing in the beginning to the quality of the paint and the putting of it on in the end, to the lowest possible notch. And while two buildings may be erected, of exactly the same dimensions, and each apparently answer the purpose as well as the other, yet there may be as much difference between them as there is between a modern automatic cut-off engine built by a strictly first-class firm, who will provide good workmanship, generous proportions, and ample strength, and an engine built only to sell by some cheap firm who cut everything down, and cover up with a gaudy coat of paint the material and workmanship that, in its nakedness, even they would blush to own. In this instance the two buildings, like the two engines, will strike the average beholder as about the same, and yet there may exist between the structures as great a difference as between the two engines; and, just as the high-priced engine will go along, year after year, costing but very little for repairs, and standing right up to its work, and comes from its builders well-furnished with lubricators, valves, and all the little fittings that belong to it, will compare with the light and cheaply-built engine that gets out of line, springs and breaks down on the slightest provocation, and is sent out by the builder without the necessary trimmings that the purchaser expects, as a matter of course, go with the engine, but which he finds he will either have to pay for as an extra or go without; but still, each engine will, perhaps, have the same sized cylinder and fly-wheel, and when set up at the first start one engine will apparently do the work as well as the other; so, the two buildings, at first, will answer the purpose equally well. But suppose that, in these buildings there should be cranes and shafting; while the light construction may stand the strain at first, after a short time the frame begins to rack and twist, the bracing stretches a little, and the cranes and shafts begin to get out of line, and when that once starts, it proceeds with increasing rapidity, and the springing of the frame necessarily causes the covering to begin to leak, and patching follows. The strength of the light sections are affected in a much greater ratio than the substantial heavy sections, and the building that at first seemed the cheapest in a short time begins to prove decidedly the dearest.

In an experience in designing and contracting for iron buildings extending over some sixteen years, the writer has often been surprised when negotiating for important structures with mill-owners and managers, whose general intelligence, one would suppose, would teach them that a good substantial building was just as much a good investment as a good engine or piece of machinery, to see them treat the building almost contemptuously, and act as though any structure that covered the desired space, and held up the cranes or machinery, no matter how small the factor of safety might be, would answer the purpose; and yet, these same mill-owners would think it an insult to their intelligence if you told them that one engine was as good as another as long as it was the same size and revolved at the same velocity under the same load. But, leaving the tribulations of the would-be contracting engineer at rest, and returning to the subject of the various points to be considered in deciding on the construction of a building, the following are the principal items that should be decided upon before asking for proposals:

Size.—The length and breadth of a building are generally fixed either by the space that there is available, or the dimensions of the machinery it has to contain. If the first case govern the dimensions nothing more need be said. If there is plenty of room, however, then the size should be very carefully considered, and after the contents of the building have been arranged, ample space should be provided to allow for access to every part. It is a great mistake to place an engine or other piece of machinery close up against the side of the building, so that a passage-way all around it cannot be had, or that shafts or pistons cannot be moved without making a hole in the side. A few feet more in length or width generally does not cost as much as the extra room is worth.

The question of height is one that sometimes is governed by adjoining structures to which the proposed building must conform. It more frequently happens that the height may be made whatever is most desirable, and in this case the tendency too frequently is to make the building too low in order to secure the greatest economy. This is a most serious mistake, for a low building is generally uncomfortable during summer, owing to its much greater heat than a higher one. If there are cranes or shafting in the mill a low building is particularly objectionable for two reasons.

1. In consequence of the belts being so much shorter, it is more difficult to keep them tight, and their life is shorter.

2. The strains on the crane bracing are much greater than in a high one, in fact, the strains in the crane bracing in the plane of the bottom chords of the roof trusses and on the sides will vary directly as the height of the posts of the building. For instance, supposing that we have two buildings, one of them 20 feet high from top of foundation to bottom chord and the other 30 feet, if the cranes in both buildings have the same lengths of jib and are located exactly the same, then with the same load on the cranes in both buildings, the strains on the cranes and bracing of the 20 foot building will be one and one-half times as great as in the 30 foot building and the destructive effect of the cranes on the building will be in exactly the same ratio. This is a point that ought to be more carefully considered than it generally is. Besides this in a high building the cranes can be designed much better and more economically. These remarks about crane bracing are intended only for swinging cranes and do not apply to travelling cranes.

When the building is intended for a boiler house a very low one is particularly objectionable as the roof trusses and bracing are sure to be in the way of the steam pipes, breechens, etc. A low building is also objectionable on account of difficulty in lighting it; where in a high one there can be large windows letting in plenty of light, good high doors and openings admitting the air freely.

Covering.—Having settled on the dimensions the next question that arises is generally, with what and how shall it be covered. The answer to this should be governed by the use the building is intended for. If it is for a rolling mill a good economical covering is corrugated iron fastened directly to the purlins and girts. For the roof a thickness of not less than No. 20 (Birmingham gauge) should be used and No. 18 is decidedly preferable. For the sides No. 22 will answer in about the same ratio as No. 20 on the roof, and for a good job No. 20 on the sides will agree with No. 18 on the roof. The purlins and girts should be spaced not further than 6 feet apart, and if a thoroughly good result is desired 5 feet is better. The corrugated iron should be fastened to girts and purlins by means of hoop iron clips spaced not more than 12½ inches apart, the clips passing entirely around the purlin and being riveted at both ends to the corrugated iron, as shown by Fig. 1.

The longitudinal seams should be riveted at spaces of about 15 inches with 6 pound rivets. The corrugated iron should be laid with 6 inch lap lengthwise of sheet and one corrugate lap crosswise for a cheap job or one and one-half corrugate lap for a good job. The best sized corrugate for general purposes and the one referred to above is 2½ inch centres of the corrugates, which are ¾-inches high. A common practice in cheap work is to use a small offset clip, shown by Fig. 2.

An equally bad construction is shown by Fig. 3, which represents a wire nail answering for both rivet and clip.

The methods shown by Figs. 2 and 3 are both thoroughly bad.

At the eaves of the building and along the edge of the ventilator is where the corrugated iron nearly always gets loose and tears up first; this is where the wind catches it and strips it from the purlins. For these places the best method of fastening is shown by Fig. 4.

This consists of a strip of sheet iron about 2 inches wider than the purlins made of No. 12 iron riveted to the purlins with ¼-inch rivets spaced 10 inches apart; to this strip the corrugated iron is riveted at spaces of 5 inches, or two corrugates, with six pound rivets.

If this work is well done, it is almost impossible for the corrugated iron to be blown off, and the superiority of the job over the ordinary method of fastening is worth the difference in the cost.

The purlins and girts should be strong and stiff, either well trussed or of such a section as to sustain without deflection the load that comes on it. Particular attention should be given to the stiffness of the purlins sideways and to the girts vertically. If they deflect from a straight line they are apt to tear the clips loose from the corrugated iron, and a leaky job is sure to ensue.

The projection of the corrugated iron too far beyond the purlins at the eaves and on the ventilator is a bad error, for it affords too great a hold for the wind, and is apt to get out of line and become ragged. When it is desired that the eaves should project any considerable distance, it is best to carry the eave rafter well out and place a purlin on its extreme end than to let the sheets project. See Figs. 5 and 6.

Galvanizing the corrugated iron adds greatly to its life and, at the same time, to its cost. Where the iron is subject to the action of gases or dampness, such as the roof of a cast house to a furnace, the extra cost is more than made up in the durability of the iron. Where a slight condensation is objectionable, such as over fine machinery or other articles that moisture would injure, corrugated iron alone should not be used for a roof covering, as it is very liable to sweat when exposed directly to the atmosphere on both sides. Where wooden purlins and girts are used for the sake of cheapness, the iron should be clipped to the purlins, as shown by Fig. 7, or nailed, as shown hy Fig. 8.

The advantage of the fastening shown by Fig. 7 over that shown on Fig. 8 is that it allows the iron to expand without tearing the rivet-holes in the corrugated sheet, as the clip gives slightly.

Wooden purlins are objectionable in an iron roof, and should not be used unless absolutely necessary. They are very little cheaper than iron or steel, and add greatly to the risk of fire. An excellent covering for a roof, and one that is strictly fire-proof, consists of slates laid directly on to iron purlins, to which they are held by copper or galvanized wires.

It is, however, very expensive, and from its great cost not to be recommended for ordinary purposes.

Slates are frequently laid on nailing strips fastened to wooden jack rafters resting on iron purlins, as shown by Fig. 9. This, while offering some advantages, is very objectionable on account of liability to fire; it is little better, in this respect, than an ordinary wooden roof, even if the trusses are of iron.

A third form of construction, and one very suitable for buildings containing machinery, consists of a course of thick planks nailed to strips that are bolted to the purlins and rafters. The planks should be surfaced on one side, so as to bring them to a uniform thickness, and then laid with the smooth side down so as to better receive the paint. The plank should be splined or grooved on both edges, and, when being laid, there should be fitted into the groove a tightly-fitting spline or double tennon. The covering, then, can be fastened directly to the plank. A good slate laid on two thicknesses of tarred felt paper makes the best and most durable covering that can be had, and next to this would come a first quality of roofing tin, put on with a standing seam up and down the roof, and a flat lock seam lengthwise of roof. The tin should also be laid on two thicknesses of tarred felt paper. After tin, corrugated iron on felt paper can be used, and, last of all, a gravel or tar roof. In making a tar roof, care should be taken not to give too great a pitch to the roof, for in that case, when the roof gets very hot in summer, the covering is liable to creep or flow down to the eaves. No more than one and one-half inches slope per foot should be given to a gravel roof. A slate roof, however, can scarcely have too great a pitch, and should never be made with less than four inches per foot, or one-sixth of the span. A tin roof may be made of almost any pitch. Corrugated iron should not be laid with less than four inches pitch per foot, and more than eight inches is not economical. Where a roof has cinders falling on it, such as a converting mill, it adds greatly to its life to increase its pitch to thirty degrees, or even one-third of the span, as this causes the cinders to slide off. For a plain corrugated iron roof, a pitch of one-fifth or one-fourth of the span will generally be found the most satisfactory. For a rolling-mill, corrugated iron has probably more advantages than any other material. It is cheap, and when kept painted is reasonably durable. It is also very light, and can be secured to the frame-work readily and in such a manner as to admit of its removal should it be necessary.

The covering of the sides of a mill admits of nearly as much variation as the roof. Where a great amount of air and light is necessary, such as in a rolling-mill building, converting-house, or melting-room, probably corrugated iron possesses the most advantages. It can be secured to the side framing in such a way as to leave a clear opening all around the sides with the exception of the main posts of the building, which is not the case with brick or stone; they require pilasters at each post to support the arches or lintels that carry the intervening curtain wall. Corrugated iron can also be readily framed around window and door openings; and on account of the ease with which it can be removed, it is frequently quite advantageous.

The remarks about fastening the clips and riveting the seams of roof iron will apply equally to the side covering except that the riveting strip at eaves is not necessary. Care should be taken to nail the iron securely to the window frames and to flash well over all openings.

A very good way to finish the sides of a mill building, where it is desired to have a large amount of opening at some times and to close them at others, is shown on Fig. 16.

This represents one side of a warehouse building of the Oil Well Supply Company of this city. The whole length of one side of this building, 260 feet, is fitted with lifting doors sliding between the main posts and so arranged that they leave a clear opening of 10 feet. This is frequently a great advantage where, as in the case of this building, it is desired to load material on a track alongside of it, and at the same time to be able to close it up in stormy weather.

Another way of closing up openings on the side of a building is shown by Fig. 17.

This consists of wooden doors hooked on to the lower girts; these can readily be detached and laid away. Wood is much better than corrugated iron for this purpose, as a very slight blow dents and injures the latter; wood can be repaired easier, is cheaper and lighter. The boards should be nailed to the frames at an angle of 45°, as shown on the sketch in order to brace the door and keep it square. Care should be taken not to make the doors too large, as a small one can be handled more readily than a large one and is easier on the fastenings. As a general rule the size of portable doors should not exceed say 8 feet in height by 10 feet in width. A very good size of stuff for the same would be 2 inch by 8 inch oak for the frames to be halfed and bolted together at the corners, fitted with tight iron gussets, say $\frac{3}{16}$ inch and $2\frac{1}{2}$ inches by $\frac{1}{2}$ inch iron hangers. The covering to be of 1 inch pine surfaced to $\frac{7}{8}$ inch, tongued, grooved and beaded well nailed to the frame.

Where doors are not provided and the sides are left open, 8 feet from the ground is a good height to stop the sheeting; if a higher opening than this is left the rain and snow will blow in, and less than this will not give as much light and air as is frequently desired. A plan that secures quite an important addition to light and ventilation, where it is not necessary to provide for the exclusion of cold air, is to let the eaves of the building project well over the sides and then stop the sheeting about 18 inches or 2 feet below the covering of the roof, leaving a clear space for air and light.

Where permanence and goodness are desired without regard to first cost, and where a great amount of light and air is not absolutely necessary, either brick or stone unquestionably makes the best material for side and end walls. There are cases, however, in which brick or stone will not do without some other support, such as iron or steel frame-work.

I refer now to buildings that have in them large swinging cranes. To properly provide for the strains caused by cranes there should be independent posts extending from the foundation to the roof, thoroughly braced to the trusses and to each other so that all of the strains of the cranes will be transferred directly to the foundations, without in any way affecting the walls. This is an important point that is often overlooked, and as a consequence where independent posts and bracing are not provided and the strains are transferred directly to the walls, the cranes in a short time, unless the walls are very massive and designed with the resisting of the cranes in view, will begin to rack and disintegrate and the cranes gét out of line.

Where cranes of any kind, either travelling or rotating, are introduced into a building, and it is desired to use brick or stone for the side walls, the best plan is to frame the building entirely independent of the side covering, putting in all the bracing necessary to withstand the cranes; then after the frame has been tightened up, have the cranes worked for say a month or two before the brick is built up; then go all over the bracing, tighten it up, adjust it and finally build up the brick, merely as a curtain or screen between the posts. When this course is followed, a very light wall, say 8 or 9 inches, will answer quite as well as a thick one. An excellent way of framing the gable ends of a building when there is any reason to suppose that it will ever have to be extended is to make the end corner posts the same as the intermediate side posts and place a truss across the end, then frame in from the bottom chord of the truss to the ground wooden or light iron posts, merely bolting their tops to the truss and bolting the horizontal girts to them and filling in between them as much space as you can with windows, the same size that are used on the sides. Then whenever it is desired to extend the building the temporary posts and girts can be readily removed and the truss being already it will be a very great convenience. The windows that were in the temporary end can be placed in the new sides and the whole operation of enlarging made much cheaper by the exercise of a little forethought.

The proper amount of light to give a building is also a question that requires considerable thought. While sunlight is unquestionably cheaper than artificial light, still there are several very serious objections to too many and too large windows. In the first place around a mill they are very apt to get broken, and of course the larger the number of windows, the greater the liability of breakage, and to a manager who cares anything for neatness, broken panes of glass are very annoying. A very good rule of thumb for the amount of windows required for an ordinary rolling mill building is to make one-fifth the entire side of the mill glass. For instance, if a mill were, say 20 feet high with trusses spaced 20 feet apart, giving 400 feet area on each bay of side covering, two windows in each panel 5 feet wide 8 feet high, would give 80 feet window surface and light up an ordinary mill building. Of course this rule would not apply where two or more roofs are placed side by side. In such a case skylights or roof windows should be provided. Where skylights are used, unless there is some special reason to the contrary, a good place for them is about half way from the eaves to the peak of the roof.

In a mill building the frame work and sash bars of a skylight should always be made of galvanized iron, as wooden frames are apt to get so warm and dry that they shrink and warp. Where roof windows are used, they are generally placed on the sides of a monitor or ventilator. This should be of ample width so that the light from the windows can strike down into the mill. A narrow ventilator with little windows that are only a few feet across from each other is an invention of the adversary, and to be shunned by all right-minded mill men. Let your monitor be not less than one-fourth the width of your main roof and of a good height, and you will secure the object that you placed it there for, if the object be light. A very wide monitor does not ventilate a mill as thoroughly as a narrower one, so where both light and ventilation are desired, it is well to make the ventilator very wide, and then put either an auxiliary monitor along the ridge, or else a series of independent ventilators.

The reason that a wide monitor does not thoroughly ventilate a roof, is that it forms a pocket at its apex for hot air to collect, without any chance for its escape. A narrow ventilator is not open so much to this objection, and the wind blowing readily through from one side to the other makes a draught and sucks up the foul air or gases much better than in a wide one. This is shown by Figs. 14 and 15.

An excellent arrangement of the sides of a monitor is to make one-half of the sash so that they will slide behind the other or fixed half, and provide a walk-way for access to them on each side, on either the outside or the inside of the roof, the latter being preferable.

Where a great deal of ventilation is desired, it may be accomplished in one of two ways, both of which have been found to give excellent service. The first consists of forming two entirely independent ventilators the whole length of the roof, as shown by Fig. 11. A second way consists in making each purlin that supports the corrugated iron in the roof double, and arranging the corrugated iron in separate and independent courses, as shown by Fig. 10.

This probably gives for such a purpose as a cast house for a furnace the best roof that can possibly be made. It is, however, very expensive, owing to the extra material and the great amount of labor required, but it is worth the extra cost.

When all of the above points have been considered in blocking out the design for a building, there still remains one to be decided, that presents to the engineer really the greatest problem of all, and that is how strong shall the building be made.

Strength requires material, and material means money, and as that is generally what the owner parts with in as small a quantity as possible, the man who decides how strong the building shall be, has generally to steer between the Scylla of a high price and the Charybdis of a light poor job, and it is frequently no small matter to keep one's head clear and stear on the straight and narrow course that leads to a medium-priced and satisfactory structure. As the circumstances that surround any particular building may vary so much, and particular causes arise that would very properly modify any design and upset any definite set of rules, the writer can only give what in his judgment are about the best limitations for strains and loads. The unit strains that he recommends may seem very low to bridge engineers who are used to a strict inspection of material and a refinement in calculations of strains, together with a high standard of material and workmanship. The great majority of the iron buildings that are erected are for men that know nothing about inspection or the quality of material proper for a building; they sometimes know what loads may come on a building, and are competent to give a fair opinion as to what factor of safety a building should have, but such cases of enlightenment are very rare, and when they are met with it is really a pleasure to the designer, for it insures him a fair comparison of his plan with that of his competitors.

The purchaser of a building generally relies altogether on the honesty of the contractor, and even if the latter should be that "one man in ten thousand" that Hamlet alludes to, still he is not going to note on his proposed plan or state in his specifications that all the material he proposes to furnish shall have the ultimate strength, elastic limit, elongation, etc., called for by a first-class bridge specification. No; he frequently ignores all these bothersome and necessarily expensive little niceties, and says nothing about them. Should the contract be awarded him, any material he may have in his stock-yard that is of the right size is used, without any question as to its pedigree or its present condition, and the rest of the material is ordered from the mill that gives him the lowest quotation, for as a general rule men do not build mills for their health. And should the case be, as it often doth befall, that the job goes to the very lowest bidder, and he has been cut down in price and had a lot of extras worked on to him without any increase of pay, then he naturally looks around in every direction to save both material and labor, and "need's must when the Devil drives" is the excuse for things that would make a strict bridge inspector's hair stand on end; the driver being, in this case, the man who has cut everything down to the lowest possible notch, and then expects to get a fine substantial building.' So taking all these points into consideration, and remembering that cranes are apt to be over-loaded often far beyond what they are originally intended for, and that one piece of machinery after another is frequently piled into a building, and bearing in mind the frailty of the average contractor and his proneness to make the cheapest do, the following are what the writer would recommend as the specifications for an ordinary mill building, and will give all the points necessary for the competing bidders to work on the same basis.

1. *Location.*—State nearness to railroad and name of road; also, what hauling may be necessary.

2. *Dimensions.*—Give width, length, heighth to eaves, pitch of roof, number and length of panels for framing. For the latter the writer would not recommend more than 20 feet in length or less than 15 feet, the most economical length being near 20 feet. State if the roof is to be a clear span or if posts may be introduced. A simple sketch giving outline of building is a great help to clearly comprehend what is wanted.

3. *Foundations and Floor.*—State if these will be furnished by the owner or by the contractor. It will nearly always be found advisable for the owner to provide both, as he being on the ground knows better what they will cost, and they can be done under his own inspection. The iron contractor should furnish a foundation plan showing the setting of anchor bolts, spacing of columns and exact sizes of bases of same. He should also include in his proposal for the iron work, the furnishing of the anchor bolts and washers; this should be noted in the specifications. A very good reason for the owner's furnishing the foundation himself, is that when excavating for the same, bad ground, quicksands, etc., are frequently struck, and the size of the piers and walls have to be greatly modified by what develops as the foundations proceed. Foundations being always more or less a matter of uncertainty, a contractor naturally bids high enough on them to secure himself from possible risk, and the owner will save money by providing them himself.

4. *Covering.*—State clearly what the roof and sides will be covered with, what projection is wanted at eaves, if cornices are wanted on gables or sides, and if gutters and conductor pipes are required, and of what size. When two roofs adjoin each other, as shown by Fig. 18, the gutter should be of ample size, supported with three purlins; it should be made of not less than No. 16 galvanized iron with riveted and soldered joints, furnished with large conductors at short distances apart. By putting three purlins under a gutter and well securing the same, a man can walk

along it and shovel out snow or other accumulations. Valleys are a great source of trouble on roofs and should be avoided wherever possible. A steam-pipe fastened directly under the centre will allow the gutter to be kept free from ice during the winter.

5. *Windows and Doors.*—Give the number and size of both; if lifting, sliding or swinging doors are wanted state the requirements.

6. *Ventilators.*—Give width and heighth; if a monitor is wanted, or if round ventilators are preferred; give number, size and thickness of iron in same, taking care that too light a thickness is not used. For ventilators 30 inches in diameter No. 16 will do; 4-foot ventilators should be made of, say, No. 10; larger round ventilators than 4 feet are not recommended, it being better to use more of them than to increase the size. The hoods or roofs of ventilators should project well beyond the body of same, to prevent rain from beating in. If a monitor ventilator is used, state whether the sides are to be open or to be fitted with slats or louvres, giving the material that they are to be made of. No. 18 iron makes a good ventilator slat. These should be turned up and down at edges, as shown by Fig. 12, and well secured or they will get out of line.

7. State if ends are to be framed for temporary or permanent gables; if the former, state if wooden or iron sub-posts are to be used.

8. State clearly just what loads the building will have to sustain. If there are to be swinging cranes, their position should be indicated on the sketch accompanying the invitation for proposals, and on this should be noted the extreme length of jib and the maximum loads of the cranes, and the bracing should be calculated for all the cranes being loaded to their full capacity at the ends of the jibs, and all swinging in the same direction simultaneously.

If a travelling crane is to be used, a sketch giving the clearance required, both over the tops of the rails the crane runs on and the distance from centre of rail to face of post, together with an end view of crane, giving the diameter of truck-wheels and the maximum load that can come on the crane, taking care to add the weight of one-half the crane girders and all the weight of trolley and the load to be lifted, together as the load on one end, should be given. If shafting or other loads are to be sustained in the building, give their position, weight and such other data that would affect the calculations of strains. To the weight of cranes, shafting and other internal loads of building, there should be added for each square foot of roof surface, measured on the angle of roof, say 30 pounds; this will amply cover all wind and snow loads for a roof of moderate pitch. For a flat or gravel roof, where the snow is liable to bank up, 60 pounds per square foot is not too much to allow. For the wind pressures on the sides of a building and the vertical projection of roof, a load of 20 pounds per square foot will provide for ample strength of framing, and the posts and girts should be calculated for the bending strains caused by such a load. In addition to the strains of cranes, machinery, wind and snow, the trusses and columns must sustain their own load. This, of course, varies with each particular case, and what the allowance should be can best be provided for by the designer; it should, however, be mentioned in the specifications to be included in the calculations.

For a rough approximation, iron roofs from, say, 40 to 80 feet span, will weigh about 8 pounds per square foot of ground covered; from 80 to 100 feet span, about 10 pounds. This is exclusive of any crane bracing, or any sides or ends, and is only for a roof ready to set down on columns or walls. About 6 pounds per square foot of sides and ends will cover the weight of an ordinary building up to, say, 30 feet high, not including any crane bracing or crane girders.

9. If top steadiments for swinging cranes or run-ways and rails for travelling cranes are to be included in bid, state it, and give sketch of what is required.

10. State what factor of safety the building is to have, giving the maximum strains that will be allowed in any part. On this point there exists such a diversity of opinion, and there may be circumstances that would so largely alter any particular case, that what follows may be taken only as the writer's opinion in regard

to mill buildings generally, and not by any means as of universal application.

For buildings without any cranes or machinery, the following loads added together: Wind and snow, say, 30 pounds per foot, with wind pressure on sides and ends, and the dead load or weight of the building should not cause a strain in any part of the frame work to exceed the following limitations:

Tension on wrought iron per square inch of net section, 12,500 pounds. Tension on steel per square inch of net section, 15,000 pounds. Where material has to be welded, as in loop eyes, wrought-iron should always be specified.

Shearing strains on wrought-iron rivets and bolts 6000 pounds per square inch, and on turned pins and bolts 7500 pounds per square inch.

Shearing strains on steel rivets and bolts 7500 pounds per square inch, and on turned steel pins and bolts 9000 pounds per square inch.

Compression strains on wrought iron members where the length does not exceed twenty times its least diameter, 8000 pounds per square inch of gross section; from twenty to thirty diameters a reduction pro rata from 8000 pounds to 5000 pounds per square inch.

For steel compression members whose length does not exceed twenty diameters, 10,000 pounds per square inch of gross section; from twenty to thirty diameters a pro rata reduction of strains from 10,000 pounds to 6500 pounds per square inch. For compression members whose length exceeds thirty diameters a still greater reduction of strains until forty diameters is reached, which ratio of diameter to length should not be exceeded, and when forty diameters is reached the compression strains should, in no case, exceed 3000 pounds per square inch.

Where compression members are subject to a bending strain in addition to their initial strain, such as is brought on the rafters of a roof truss by the purlins, additional section should be provided to compensate for this strain. It will be noted that no distinction is made between the pin-ended or square bearings, for in the writer's opinion the workmanship on the average building is such and the foundations on which the columns rest may be so out of level and uneven that it is best to class all compression members under one head and pay no attention to theoretical differences in value in pin and square bearings, which are largely imaginary at the best.

For buildings which sustain crane or shafting strains a larger factor of safety should be provided. For these the writer would recommend the following:

First. For the trusses, purlins, gable posts, travelling crane girders and girts, the loads of the wind, snow, weight of building and crane loads, as before specified, added together, and these not to cause any strains to exceed the following limitations:

Tension on wrought iron, per square inch net section, 10,000 pounds. Tension on steel, per square inch net section, 12,500 pounds. Shearing on wrought-iron rivets and bolts, per square inch net section, 5000 pounds. Shearing on wrought-steel rivets and bolts, per square inch net section, 6000 pounds. Compression on wrought-iron under 20 diameters, 6000 pounds. Compression on steel under 20 diameters, 8000 pounds. Compression on wrought-iron from 20 to 30 diameters from 4000 pounds to 3000 pounds. Compression on steel from 20 to 30 diameters from 8000 pounds to 4000 pounds.

And no compression member to have an unsupported or unbraced length, under any circumstances, to exceed 30 diameters. For the crane bracing, shafting supports and the main columns to the building a reduction of the above strains to one-half. In all cases the end connections of every member of the trusses, framework, bracing and girder to be fully 20 per cent. stronger than the body of the member; that is, the rivets, pins and bolts forming the end connections are to be calculated for strains twenty per cent. greater than the body of the member, so that failure will not occur at the connections. This is a very important point and should be looked after more closely than it usually is.

After the maximum strains are specified, the kind of material to be used and the quality of the same should be named. Good ordinary building steel should be capable of standing the following tests:

Ultimate strength not less than 50,000 or more than 62,000 pounds per square inch, with an elastic limit of from 26,000 to 30,000 pounds per square inch, and an elongation of from 12½ to 15 per cent. in 8 inches.

Building iron should have an ultimate strength of from 45,000 to 48,000 pounds per square inch, an elastic limit of from 24,000 to 28,000 pounds per square inch, and an elongation of from 12½ to 15 per cent. in 8 inches.

Rivet iron and steel should bend cold over a circle of its own diameter without fracture. It is usually best to let the builder decide for himself whether he will use iron or steel, the difference in the market price generally governing his choice, and by leaving this point to his discretion the owner of the building gets the benefit of whatever difference there may be.

11. *Kind of paint to be used and number of coats required.*—For ordinary cases, one coat in the shop and a second coat after erection will be sufficient, and any good iron oxide paint mixed in boiled linseed oil will answer. Care should be taken that surfaces in contact are given a good thick coat before they are riveted up; for special cases lead or other paint may be necessary and if so, it should be noted in the specifications.

12. What is required in the proposal should be clearly stated in the invitation for proposals.

While the intending buyer has a perfect right to know exactly what he is going to get for his money, still the writer does not believe that it is fair and right to ask a number of competing bidders to each submit an elaborate plan for a building and then give the contract to one bidder and let the others lose the cost of their plans. It may be answered that if they get up plans they do so at their own risk and that they did not have to; while this is true in one way it is not true in another. If proposals are asked for from several bidders and one party submits an elaborate and handsome plan for the work and the other bidders only single line diagrams unless the man to whom the plans are submitted is quite out of the ordinary run of building owners, if the bids are anywhere near alike the one that puts in the handsome plan will get the contract nine times out of ten, and as iron building manufacturers are only too sadly aware of this fact, the cost of getting up proposal drawings is a very large item in the year's expenses. It seems to the writer that if in the invitation for proposals, it was stated that only single line diagrams, having all the sizes and strains of the various parts, with sketches of such details as are absolutely necessary to understand the construction, were desired or would be considered, that all parties would be treated in a much fairer manner than is at present the case. In the proposal for the work each bidder ought to be required to give the estimated weight of the material he proposes to furnish, with the express understanding that any failure to furnish within a close percentage the amount of material he proposes will entitle the owner to a corresponding reduction in the price.

Having the estimated weights that each party proposes to furnish clearly before him, the intending builder can make a fair comparison of various bids, and judge more intelligently of the merits of the different plans.

The time that the building will be needed should also be specified, and as time work only too frequently means slighted work, and is generally much more expensive than work that a reasonable amount of time can be given to, when a new building is contemplated the matter should be taken up as soon as convenient, and as much time allowed as is possible. It should also be remembered that nearly always buildings can be erected cheaper and better in summer than in winter.

Lastly.—An item of considerable interest to the contractor should not be omitted from the specifications and that is the terms of payment. This is too often omitted altogether and left for an understanding to be arrived at after the contract is awarded. Certainly if the owner of a building has a right to know just what he is going to get for his money, the contractor has an equal right to know just when and how virtue will be rewarded.

DISCUSSION.

PRESIDENT HUNT: I think the unit strains that Mr. Seaver has called for in his specification are reasonable and they are not as heavy as I expected that the author as an engineer who is designing many buildings in active competition would recommend.

I am myself in favor of allowing the heavier unit strains, bearing in mind what that means. It means lighter sections and a lighter building. I do think we can safely strain good soft steel in ordinary buildings, under favorable conditions, in tension, up to 15,000 pounds per square inch; and in fact, where the sections shall in all cases have a minimum of one-quarter of an inch thickness, under favorable conditions, that he could allow loads on an ordinary building perhaps 10 per cent. more than he has called for of 15,000 pounds per square inch unit strains for steel.

In his specification for steel, his requirement for tensile strength runs from 50,000 to 62,000 pounds. The soft basic open-hearth steel now being made by Carnegie, Phipps & Co., Ltd., has, some of it, run as low as 48,000 pounds. I would not advise rejecting that soft steel because of its extreme purity, for that is what it often means when it is so low in tensile strength; and when the steel has extreme ductility, I would suggest a lower ultimate in the steel, going to 48,000 pounds instead of 50,000 pounds.

I would, however, require a considerable increase in the ductility requirement from what he suggests. If I heard aright, he called for 12 to 15 per cent. in 8 inches. I think he can and ought to require of that steel an elongation of at least 20 per cent. in 8 inches.

Another point with reference to his paint; I am glad to have him call attention to the fact that there is poor paint. That subject I have had specially called to my attention lately. Iron-ore paint is something that is terribly adulterated in these days. I have had in our own laboratory tests of so-called iron-ore paint that ran all the way from 5 per cent. oxide of iron up to the requirements of the ordinary 40 to 50 per cent. oxide of iron. The adulterant that is most used and the worst to iron ore paint is carbonate of lime. It is ordinary limestone ground up. It is cheap and it is poor. It decomposes and it is one of the causes of poor paint to-day. The best material ordinarily used as a pigment with oxide of iron is sulphate of lime. An iron-ore paint should contain at least 40 per cent. oxide of iron, the remainder being sulphate of lime. In some places they are using sulphate of barium, which is not a bad material, except that due to its heavy weight it has not as good a covering power per pound. But there should be in good paints at least 40 per cent. oxide of iron although many of the cheap paints run but 25 per cent. and some of them have as little as 15 per cent. oxide of iron.

Mr. Kauffman: I would only remark that I agree about the unit strain; I think if you know what the material is you can safely load it up to at least one-half its elastic limit, when you consider the worst cases of loading. Of course if you do not know what the material is I would not insist on this; a lower unit strain should be adopted, but I do not see how you can arrive at any conclusion as to what unit strain can be adopted without knowing what you have in the material.

Mr. Johnson: My ideas agree with those advanced by Mr. Kauffman, that where you have no knowledge of the material the unit strain is at best a guess. I do not think any material ought to be adopted for any structure without such inspection as would give you a reasonable knowledge of its character and quality.

Mr. Hutchinson: There is one point that struck me as being erroneous. Mr. Seaver spoke of making the building high to decrease the stress on the columns but that would make an increased load on the crane bracing and that would bring the strain back into the columns.

Mr. Seaver: That objection is plausible but it is not really true. I do not think the bending strain of the cranes ought to be taken up by the columns. If it was then the longer the column was the greater the leverage would be, and the worse the effect of the crane would be. In a properly designed building the crane strains will be taken up by diagonal bracing, transferring that strain directly down to the foot of the post. If we leave it unsupported then it would be certainly a valid objection, but if the posts and cranes are braced as they ought to be they will take their share and I do not think this is a well taken point.

Mr. Lewis: In almost all cases it is possible to brace with the foot of the column along the side at right angles with the trusses, but in the plane of the trusses you are generally at the mercy of the knee-brace.

Mr. Seaver: You can transfer the strain at the ends.

Mr. Lewis: In a great many cases it is impossible to take care of it at the ends. There you take it up by some kind of a sub-brace or post arrangement which does not admit of very good bracing, or you have a brick wall or something of that kind to which you cannot readily attach. I think that in the majority of cases it is necessary to take up the strain from the cranes by knee-braces, or let it take care of itself. You must have clear space through the middle. If the ends are too far apart there is too much deflection in the building, or even if you put any extra material to carry the strains from the canes to the ends of the building there is too much deflection in the frames to stiffen the cranes. The most economical road to take up the strain is the shortest distance between two points, and the sooner you get the strain from the top of the crane to the base of the pillar the better, and the stiffer the structure is the better, but in almost all cases that means in one direction, and you are at the mercy of the knee-bracing.

President Hunt: In ordinary mill construction that is the way it is done; the particular columns which are connected to the stiff leg or tension rods of the top of the cranes are increased in section to take care of the extra bending strains placed upon them.

Mr. Seaver: That is the way it ought to be, but unfortunately it is not. The posts are all made alike nine times out of ten. They are designed with the beautiful idea that the strains of the cranes are distributed so that each post takes a share of them and helps the others. That is the hypothesis on which they are made, but it is not the right way. It is the way we have to do in competition.

Mr. Hutchinson: Mr. Seaver speaks as though the increased depth of the sway-bracing would be an extra expense. As the stretch increases in direct proportion to the length of it, the increased cost of the sway-bracing would be in proportion to the height, and the increase there would be almost nothing.

Another thing he referred to was the bracing going to the ground. So far as I know in the majority of mills the sway-bracing, even on the ends, stops when it gets down within eight or ten feet of the ground, and you must allow for the wind stresses.

I would like to ask Mr. Seaver a question; that is whether he puts in a stiff member in the truss connecting the top chord to the bottom?

Mr. Seaver: Yes, sir. You refer to what is generally a tension member. That ought to be made a stiff member. We make it a stiff member so as to have it resist compression as well as tension.

Mr. Dempster: I do not profess to be an expert in that line but there is one thing that struck me more than anything I have heard in this Society for a long time, since the time, if you will remember, Mr. President, when we had a discussion on steel. When we wound up we did not know where we were. We are just in about that shape now. That is it strikes me so. There is so much indefiniteness about this. I am surprised that an engineer would advocate in any way the idea that there be competitive plans submitted for a building of this kind. Every company, every man, every mill man or manufacturer who wants a building or structure ought to have an engineer. There should be certain requirements of material and they should be succinctly stated in short specifications and the engineer should see they were carried out. We should pursue a course or advocate such a course of procedure as would give the engineering profession that prominence it ought to have, and not place it on the level that Mr. Seaver has advocated.

Mr. Seaver: That is not what I advocated. I said that competition should call for single line diagrams, with sizes and all information marked on same; in other words, a strain sheet. That would convey the idea as well as an elaborate drawing.

Pres. Hunt: As I understood it in the first of the paper in which he spoke of the two modes of giving the information with reference to getting bids, Mr. Seaver did not particularly recommend either but stated the facts, which are cold facts of business. That is the way it is done to-day. I know, as a Society of Engineers we would recommend all parties building to have an engineer and that engineer should prepare careful drawings of just what he wants and draw up careful and elaborate specifications describing what is wanted, but that, unfortunately, is not the

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position that is taken in the majority of jobs that are let to-day. It is not only so with building work but it is getting more and more so with bridge work. Parties desiring structures of iron or steel simply describe what they want in as technical terms as they can and ask for designs from the bridge companies or construction companies that are to build or bid. Mr. Seaver has given in his paper a valuable lot of data to aid such people.

I do not understand his paper as at all condemning the system of the work first being designed and working plans and specifications being gotten out by an engineer. From my own experience I know that reputable builders are always glad to have an engineer to whom their bids are referred, in competitive lettings, some one who is able, honest and willing to recognize the worth of the work when it is presented.

The great trouble is that some dishonest contractors or their agents, what are called " Highway Men," will so describe a poor, cheap design as to make it appear to the uninitiated better than one which is really a good one, so that the honest contractor is glad to have an engineer back of him to approve a good job when it is completed.

Most assuredly plans for structural metal work should be submitted to an engineer in all cases; on this point, I do not think Mr. Seaver would disagree with us.

Adjourned. R. N. CLARK,
Secretary.

MEETING OF THE CHEMICAL SECTION OF THE ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

PITTSBURG, MAY 24TH, 1892.

Chairman J. W. Langley and twenty-eight members were present.

Final report of *Committee on Handy's Phosphorus Method* showed that it would be necessary to modify the procedure when analyzing ferro-manganese.

The Nominating Committee made the following recommendations for officers of the Section to serve until January, 1893.

Chairman, John W. Langley; Vice-Chairman, Charles B. Dudley; Secretary, James O. Handy; additional directors, James M. Camp, George A. Marsh.

These nominees were duly elected.

Mr. Van Gundy read a paper on "Manganese Estimation in Steel and Iron by Textor's Method Modified." The paper was discussed by Messrs. Langley, Handy, Johnson, Van Gundy and Carnahan.

Chairman Langley then described a new method of estimating copper in iron or steel.

Mr. Handy spoke of difficulties encountered in the analysis of tin-plate, and how they had been overcome.

R. N. Clark, Secretary of the Society, explained that no member was entitled to receive the publications of the Society until his dues had been paid. The Society had been gratuitously distributing the copies of the Proceedings of the Chemical Section to the chemists as soon as elected. This free distribution would now cease.

Continuation of the Report of the Committee on James O. Handy's Rapid Phosphorus Method.

Mr. Crabtree: The point brought out by Mr. Barrett was that he found the results by *oxidation with permanganate* in the phosphorus determination, were lower in the case of ferro-manganese or spiegeleisen than by *evaporation to dryness*, and he suggested that the Committee try some more determinations. I made a number of determinations with a sample of ferro-manganese furnished by Mr. Barrett, and I have obtained the following results in phosphorus:

Evaporation Method.	Handy's Method and Permanganate Titration.	Chromic Method.
.245	.232	.203 .223
.244	.215	
	.217	
	.212	
	.240	
	.230	
	.217	
	.212	
	.215	
	.224	

My two results *by evaporation* were, I think, about correct. The results in the second column I obtained by the use of permanganate for oxidation dissolving the manganese oxide by the use of sugar, and titrating either by permanganate or by Handy's method.

The .223 which I got by chromic acid, I think is rather high on account of incomplete washing, and so I am not absolutely certain of that result.

Mr. J. M. Camp: The sample of ferro-manganese used by Mr. Crabtree was from a lump about as large as an egg. It was broken up completely, so that there was no chance for varying results. We got .245 and .246 per cent. phosphorus *by evaporation.*

Mr. R. B. Carnahan: In a test by evaporation I made on the same sample .243 and by the permanganate .223 and .230. I did not try the chromic acid method.

Mr. J. O. Handy: I made some experiments with ferro-manganese which apparently was from the same lot. I got .242 by evaporation to dryness and .216 by oxidation with permanganate. I tried some very high carbon steel and found that the results were identical whether I oxidized with permanganate or evaporated to dryness. I tried steel (I do not know whether you would call it steel) with 2.85 per cent. of combined carbon and .078 per cent. of phosphorus. I got the same results by three methods, the third consisting in weighing the yellow precipitate. I tried another steel with .95 carbon and .137 phosphorus. I got .138 by my new method, and the same *by evaporation* to dryness.

Mr. J. W. Langley: It seems to be evident from the work of the Committee that although Mr. Handy's method works well upon iron and steel, it does not succeed in getting all the phosphorus out of ferro-manganese. I do not know that the Committee can add anything more to what they have already done. I think we can accept their report on this subject as being very conclusive so far as they have gone.

Mr. Handy: I wish to call attention to the fact that the oxidation by permanganate, which has been found to fail in analyzing ferro-manganese, is not an essential part of my process. I obtain the *pure* yellow precipitate by *any appropriate procedure.*

MR. CARNAHAN: One point I would like to ask about. In evaporating that ferro-manganese with nitric acid, I would get a polish on the evaporating dish like stove polish. Has anybody any idea what form of carbon that is? It dissolved completely in nitric but not in hydrochloric acid.

MR. LANGLEY: Combined carbon is generally some hydrate of carbon. It always contains the elements of water, or chlorine, or bromine, or any of the haloid elements with which it may have come in contact in the course of its liberation, and this material is completely soluble in nitric acid. There have been some analyses made showing the composition of these carbonaceous residues. They have been printed, but I can not state the authorities now. It would be an interesting point of research for some person who has the leisure to take it up.

There being no further remarks, the paper of the evening was read by Mr. Van Gundy.

NOTES ON TEXTOR'S RAPID METHOD FOR THE DETERMINATION OF MANGANESE IN STEEL.

BY C. P. VAN GUNDY.

(*Method of Textor.*)

To one-tenth gramme of steel in No. 2 beaker add 15 c.c. of HNO_3, specific gravity, 1.20; boil till the brown oxides of nitrogen are gone; add 15 c.c. of hot water, and, while boiling, introduce one-half gramme PbO_2, measured approximately on the flattened end of a glass rod. Boil three minutes after addition of the lead oxide, filter through asbestos, and wash with water containing 2 per cent. HNO_3, specific gravity, 1.20. Titrate with a solution of arsenious acid till the pink color is gone; each cubic centimetre of solution equals one-tenth per cent. of manganese.

Precautions.—The brown fumes must all be expelled before adding water, otherwise low results may be expected. The first filtrate passed through a fresh asbestos filter should be rejected, as some asbestos reduces the $HMnO_4$ solution; or the asbestos must be first treated with HNO_3 and $KMnO_4$.

For steels containing .75 per cent. of manganese, one-half gramme or more of PbO_2 should be added, and the solution, after the addition of the lead, should be boiled not less than three minutes, otherwise low results may be obtained. To obtain rapid filtration, a special filter is required. It may be constructed as follows: Fill a 2½-inch funnel one-third to a half with pieces of glass rod ¼ to ½ inch long; on this place a disk of platinum foil fitting the funnel at the point where the disk rests on the broken glass. The platinum disk is perforated, by means of a pin, over its whole surface; the rough side is turned down. Pour suspended asbestos upon the foil till a layer is formed ½ inch in thickness. When the filter becomes clogged and works slowly, the thin layer of PbO_2 can be removed by carefully scraping with a wire, a fresh surface of asbestos thereby becoming exposed.

For the arsenic solution, 20 grammes of As_2O_3 in powder, and 60 grammes of sodium carbonate, are dissolved in 750 c.c. of hot water, filtered, and diluted to 2000 c.c. An equivalent amount of sodium arsenite may conveniently be taken. Of this solution, 85.7 c.c. are diluted to 2500 c.c., and tested with a steel containing a known percentage of manganese.

Time required, ten minutes. Duplicates check closely.

I have worked the method as described with good results, but found trouble in filtering, even when the filter was carefully made as directed by Textor, the liquid passing through very slowly after two or three filtrations had been made. To overcome this, the process was slightly modified. Two-tenths gramme of the sample were taken, and after oxidation the solution was made up to 100 c.c. This was let stand a few moments, and the greater part of the almost clear liquid run through a filter under suction; 50 c.c. of the filtrate were pipetted off and titrated with the arsenious solution. The filter of glass-wool and asbestos was made in a carbon-filtering tube, and was found to serve for a number of filtrations, as very little PbO_2 collected upon it. The first portions of the filtrate were always rejected, having served to wash the filter.

I have made a few determinations showing the range of application of the method, good results having been obtained on both low and high manganese materials, as the figures will show:

	Mn., by Williams' Method. Per cent.	Mn., by Textor's Modified Method. Per cent.
Steel,	.25	.23, .24
"	.40	.38
"	.52	.52, .51, .50, .52
"	.91	.91, .92
Pig-iron,	1.70	1.71
Ferro-manganese,	80.26	80.00

DISCUSSION.

After reading his paper, Mr. Van Gundy remarked:

In conclusion, I do not say I would really recommend the method for ordinary use. My work simply shows that it will give good results quickly if care is taken.

MR. HANDY: I have been experimenting with this method at odd times for several months, and although I have not accomplished a great deal, I have determined very accurately manganese up to 1.4 per cent. in steel. I have also tried the method on pig-iron, and have compared it with the chlorate or Williams' method and with the color method. They all agree perfectly.

MR. LANGLEY: You say that the color method agrees with Textor's?

MR. HANDY: Yes, sir. My results were as follows on a sample of pig-iron:

Mn., Williams' Method. Per cent.	Mn., Textor's Method. Per cent.	Color. Per cent.
.334	.33	.35
.340	.34	

I think that the method is a very accurate one, and that it will recommend itself for special cases where very quick work is wanted.

MR. CARNAHAN: I always found the color method, if standards are chosen rightly, to agree closely, and I do not see that you can get anything that is quicker.

Mr. Handy: I would dissent from calling the color method quick. You have to wait an hour or two for the lead oxide to settle. In Textor's method the liquid does not have to be cooled perfectly nor allowed to settle any more than it cools and settles in making up to 100 c.c. I think that titration is much quicker than matching.

Mr. Carnahan: I do not have to wait an hour for settling. I can make the manganese in fifteen minutes. I use a number of test-tubes, and I can match several in five minutes. This is at least as quick as titration.

Mr. Langley: There is an impression among some chemists that the color method, while generally reliable, is apt sometimes to give some very erroneous results.

Mr. Carnhan: I think I know a reason for that. We have had some trouble in that line. For instance, you have your nitric acid solution too strong, or if sulphuretted hydrogen is in the air of the room, you will get a weak color. I find we can get uniformly reliable results if we have our nitric acid solution one part of acid and four parts of water. The color is a definite one. You get accustomed to it, and any change will be noticed very quickly.

Mr. C. E. Manby: I have made some experiments with regard to the strength of acid. I employed one-tenth gramme of steel dissolved in (1.2 specific gravity) nitric acid, and evaporated to dryness; this was done in a few minutes. Then I took up in 15 c.c. 1.2 specific gravity nitric acid and added 15 c.c. water. I thus got rid of nitrous fumes completely. I always prefer to oxidize with tetroxide of bismuth, and was very much pleased with the clearness of the permanganate color. I obtained very good results. I would like to know if any one has used the oxide of bismuth?

Mr. Handy: When the color method first came out a good many years ago the acid which had proved satisfactory was 1.2 specific gravity nitric acid, and that has been used successfully since then. In acid of 1.20 specific gravity, however, the oxide of lead settles very slowly and it is advantageous to use a weaker acid, say of 1.13 specific gravity.

MR. CARNAHAN: It would be interesting to know what action nitric acid has on manganese when it breaks up the permanganic acid on long boiling. It cannot be a reducing action, it must be an oxidizing action. But what is it?

MR. LANGLEY: Why not a reducing action? Are not nitrous fumes produced.

MR. C. M. JOHNSON: I think Drown's method illustrates that. You put into a nitric solution an excess of permanganate and it breaks up into oxide of manganese when it is boiled. We have examined the lead oxide which remains in the tube and found it contained manganese, showing there was some manganese oxide in that lead.

MR. HANDY: There must be a *reducing* action. I have added permanganate of potassium to cold nitric acid. It soon decomposed. MnO_2 was deposited.

PROF. LANGLEY: It is quite possible there is a reducing action there. It may be by the liberation of nitrous fumes. If we treat peroxide of tungsten with dilute nitric acid there is no change, but if we evaporate to dryness peroxide of tungsten and nitric acid, the former will be reduced and instead of yellow we have a greenish color. Ammonia will do the same thing.

There being no further discussion on the paper a general discussion took place.

PROF. LANGLEY said: In determining the *copper in iron and steel* if the amount of copper is quite small it may be easily overlooked; if we follow the later methods of attempting to throw it down as sulphide we have trouble on account of the great readiness with which sulphide of copper oxidizes when undergoing filtration. If now we dissolve our metal in hydrochloric acid and then boil it with hyposulphite of sodium, we may overlook the small quantity of copper again, because the cuprous sulphide is not entirely dissolved in strong hydrochloric acid. Drown limits us to the use of sulphuric acid. We have been making some experiments lately, and it seems to be proved that if the acid is sufficiently diluted and then is almost entirely neutralized with ammonia we do not seem to lose any copper whatever. The whole of the copper comes down as cuprous sulphide. There is nothing new in that, but there is a little change in the treatment which it seems to me has facilitated our work and led to a shortening of time. The cuprous sulphide is difficult to get into solution.

We have a mass of solution which we filter. The filter is washed and then heated in a porcelain crucible until the sulphur is burned off, still leaving the carbon. The filter is then tipped into a platinum crucible and the ignition finished there. Then we have only the oxide of copper. To get that into solution we add a very small quantity of carbonate of potassium or a small crystal of nitrate of potassium, which subsequently converts the cuprous sulphide into sulphate. Then if I treat this with some bisulphate of potassium and make a fusion, the copper goes into solution with great promptness and completeness. None ever escapes the action of the bisulphate, and the whole thing can be accomplished inside of seven or eight minutes. It then only remains to dissolve the mass in water and throw the copper down by battery. By this process the time is very much shortened. In our experience thus far there has not been a case where the cuprous sulphide has escaped the action of the oxidizing agent.

Mr. F. N. PEASE: Our method is similar. We have however always dissolved the copper sulphide by warming it in a porcelain crucible with four or five drops each of sulphuric and nitric acid; we filter into a platinum dish and determine the copper by electricity. The precipitation is done in the way you describe.

MR. LANGLEY: Do you have any trouble in getting your cuprous sulphide into solution?

MR. PEASE: No, we have not had any trouble. We seldom run over .05 per cent. copper.

MR. HANDY: In your experiments did you ascertain the maximum percentage of free hydrochloric acid which may be present when copper is completely precipitated?

MR. LANGLEY: We did not determine it exactly, but there would probably be not over 2 per cent.

MR. HANDY reported that in making some experiments to get the percentage of *tin in tin-plate,* where the material had been treated with nitric acid and evaporated to dryness, although he knew that tin was present (fully 5 per cent.), he did not succeed in separating any of it by this method, but did succeed in getting it out by evaporating the nitric solution with H_2SO_4 to fumes. After this he dissolved in water, and precipitated the tin completely by sulphuretted hydrogen.

MR. WILLIAMS, MR. CAMP and MR. CARNAHAN reported similar experiences.

Adjourned.

JAMES O. HANDY,
Secretary.

JUNE 21ST, 1892.

THE Society met in the parlors of the Academy of Science and Art on June 21, 1892.

Mr. Alfred E. Hunt, President, in the chair; Mr. R. N. Clark, Secretary; twenty-seven members were present.

The minutes of the last meeting were read and approved.

The following gentlemen were elected members of the Society by ballot: James W. Arrott, Jr., John W. Bailey, W. W. Card, Abram T. Eastwick, James M. Davis.

The Library Committee reported through Professor Francis C. Phillips, that nineteen books had been added since the last meeting, two book cases, and that all the books were now being reclassified so as to make them much more accessible.

The Committee on Smoke reported progress. Mr. A. Dempster resigned from this Committee; on motion the president appointed Mr. Thomas P. Roberts in his place.

The Memorial Committee on the death of Mr. B. L. Wood, reported by Mr. Alex. Dempster as follows:

Death has again invaded the precincts of our Society and taken away another of its esteemed members, and in so doing has removed another factor from earth's activities; blotted out another star from the constellation of our friendships, plucked another flower from the parterre of life's companionships, and has made it our duty to transfer the name of B. L. Wood, Jr., from the list of active membership to the honored roll of our Society's dead, round which the tendrils of affection cling with tenacious touch.

We are thus called upon to pass thro' the ordeal of sorrow, as we stand by the bier of our friend and photograph upon the camera of memory a few of the more prominent rays that shed from the cheerful geniality that characterized his intercourse with his fellows—as the sun setting behind the western horizon sheds the beams of departing glory over the landscape and clothes the surrounding summits with a golden glow—so the sun that has suddenly become obscured—ere yet it had passed the zenith of its glory—reflects a radiance over the pathway of the years o'er which it has shone—and these we focus into pleasing recollections and imprint upon memory's tablet, with the hand of tenderness—and give expression to our feelings in the pathos of the soul.

Mr. Wood was born at Sharpsburg, and spent his boyhood days within the limits of Allegheny county beneath the fostering shades of a happy home, surrounded by the nurturing influences of a noble parentage, whose best boast is manhood, dignity in unbroken chain of heredity, which is the best birthright of an American, and a legacy of worth.

He received his education in the public school—the college of the people, from which have come equipped for the battles of life so many whose names are emblazoned on the shaft of fame, and written in the records of our country's great ones. He studied for some time in the Western University, and finished his school education while still young in years.

There was little to mar the monotony of that every-day life that is the history of the most of men under such conditions, and I have no desire to burden the records with the minutiæ that go to make up the bulk of a man's record and merely say that he chose business pursuits instead of professional. He was not an engineer in the technical application of the word, but he was so identified with engineering works, that his natural ability and knowledge were supplemented by extended experience and developed into the lines of practical engineering. His horizon was bright with the prospects of business prosperity. He was liberal in his patronage of scientific purposes.

As a man he was "suave" modest and manly—and rills of beneficence unseen by the outside world flowed in unpretentious current from the sources of a sympathetic heart to bless many who have occasion to remember his deeds of kindness with the recollections of gratitude. He was not perfection, else he would not have been a man, and when we summarize his virtues and paint his manhood in the blending colors of love and affection which may seem to partake of bias, we acknowledge his weaknesses and throw over them the mantle of Charity, and hide them in the folds of those noble qualities of head and heart, which gave dignity to his character and which we cherish with fondness. And as we lay a flower of affection upon his tomb and close its portals to all that is earthly of the friend that's gone—and remember that he has passed beyond the confines of time to that far off realm (whose sealed Arcana no mortal eye can see, and where no instrument of science can penetrate—and no human conception can anticipate), his words and deeds, and influence alone now live. And as we will behold his face no more, and as we remember that we too must pass that bourne from whence no traveller returns; and "that as the tree falls, so it must lie," let this be our incentive: "to act well our part" while the sun of time and opportunity still shines and illumes our pathway—and by noble words and actions give those whom we leave substantial basis to pronounce our obituary in truth, as we do that of Mr. Wood when we say that one of earth's true noblemen has been taken away.

Mr. W. Lucien Scaife then read his paper on

SOME DATA ON RAIL-JOINTS.

During the year 1885, considerable interest was manifested in rail-joints by the members of this Society. The importance of the question was generally recognized, and it was thought that the Society should endeavor to do something towards solving some of the problems involved in the production of the best possible track for railways. Accordingly, the following committee was appointed to investigate the question of rail-joints, and to make all necessary tests of the merits of different splice-bars then in use: It consisted of Thomas Rodd, Chairman; Max J. Becker, H. W. Borntræger, W. R. Jones, C. L. Strobel, W. L. Scaife, Wm. Scherzer, James G. Dagron, and Frans Engström. Mr. Scherzer was the first Secretary, and on being called to another city, was succeeded by Mr. Engström. After some discussion, and in order to make the investigation as thorough as possible, the committee proposed:

1. To test, in tension and by bending, samples cut from each lot of splice-bars.

2. To test the splice-bars, attached to short pieces of rails, in a testing machine, by applying loads corresponding as nearly as possible with those encountered in actual practice.

3. To place a number of the different kinds of splice-bars on track, where all will be subjected to the same conditions and to note the effect of passing loads and wear of bar.

4. To test, in connection with the splice-bars, the different varieties of nut-locks.

As the Society provided no funds for carrying on the work, circulars were sent to various manufacturers of splice-bars and nut-locks, asking for samples and subscriptions for the purpose. Many generous responses were received in reply, but unfortunately the work was never advanced far enough to call for either.

In order to have the benefit of the experience of railway engineers, Circular No. 2 was prepared and sent to most of the railway companies in the United States and Canada. It contained the following questions which were believed to cover the subject pretty thoroughly:

1. What is the length of lines on which splice-bars are used?
2. What per cent. of line is on a grade, approximately?
3. Are the grades generally over or under 1 ft. to 100 ft.?
4. What per cent. of curves, approximately?
5. Are the curves generally more or less than 5 degrees?
6. What kind of ballast is used?
7. What is the greatest weight on drivers and per driver?
8. What is the distance apart of driving wheels, axles?
9. Length of Standard rail? Material?
10. Cross-section and weight per yard? (Refer to book).
11. Sizes and distances of holes from end?

12. Cross-section, and length of ties and kind of timber?

13. Distance apart centers, at joint, elsewhere?

14. Of what material are your splice-bars?

15. Weight, cross section and length?

16. Is joint over tie or suspended?

17. What per cent. of bars break?

18. Do they break on straight lines or curves, or both?

19. Do they break more frequently on curves?

20. Do they break frequently on bridges and trestles?

21. Where does the fracture take place, and how?

22. For how many years has the section been used?

23. Is it satisfactory? If not, what changes would you suggest?

24. Have you made tests of bars or material? If so please give results.

25. At what points, preferably convenient to large cities, can the Committee personally examine good examples of track put up with the joint used, that has been in service for some time?

In response to these and other circulars, as well as special letters, answers were received from engineers, general managers and superintendents of about 61,000 miles of railway. Many of them were accompanied with drawings of rails, splice-bars, road-beds, nut-locks, etc., and furnish much valuable information outside of the questions contained in the circulars. All of these original documents will be found in the library of the Society, and will amply repay a careful examination by any one interested in railroads. Owing to the removal from the city of several members of the committee, and the increasing duties devolving on others, it was found impracticable to carry out the tests, as originally intended; and meetings were discontinued after May 1888, no report being made to the Society, owing to the absence of any actual tests. It occurred, however, to the writer, that a brief summary of the answers received would have a historical value, if nothing more; and might be of service to similar committees of our own and other societies. Moreover, some explanation, though tardy, is due to those gentlemen who so courteously complied with the request of the committee, in hope of increasing the knowledge of this important subject. If we cannot give them a report as promised, we can at least offer them a summary of the replies to the circulars sent out.

These were carefully tabulated by Mr. Engström, at the time they were received, and are hereto annexed:

Table I covers 35,600 miles of railroad, and includes replies of those in charge, who were satisfied with the splice-bars then in use.

Table II refers to those who were in doubt, or dissatisfied with the then existing joints, and represents about 25,500 miles of road.

Table III has been calculated by Mr. Engström, and shows for the various railroads approximate values of the area, radius of gyration, moment of inertia, and center load at elastic limit, of the rails and splice-bars, together with their relative values.

The accompanying diagram shows the same values graphically.

An examination of these data cannot fail to show wide differences of opinion where they should not exist, and which it was the object of our Committee on Tests to settle. Considerable progress has been made since these communications were received, and the writer hopes that some of our railroad engineers will add to this imperfect historical fragment the results of their more recent experience with rail joints.

Here are the drawings referred to in the report which it is proposed to have bound and which the members can look at if they desire. Here is a book which contains properly filed the answers to the various questions which Mr. Engström has kept in very nice shape indeed, and which contain very much more information than could be put into these tables. Some of our correspondents went to a great deal of trouble in giving ideas of their own, as well as answering the questions.

DISCUSSION.

MR. MAX J. BECKER: As one of the members of the Committee on Rail-Joints, I wish to say that, I quite agree with Mr. Scaife, that some apology is due to the Society who appointed the committee fully six years ago, and also that some explanation should be made to the different parties, who, at the solicitation of this committee, contributed information and tendered their services and assistance in connection with the investigations, which were commenced diligently enough, but not prosecuted with the zeal and energy with which they were begun.

Of the nine members, who constituted the committee, the most active one, who was best qualified for its work, on account of his connection with the establishment upon which we principally depended for making the tests, died; four other members removed from the city and severed their connection with the Society. The remaining four, myself among the number, have found it impossible to devote the necessary time to the subject in their charge, which its extent and importance seemed to demand.

Moreover, the causes, which created the interest in the subject, and led to the appointment of this committee, have, in a great measure, disappeared, so that at this time even the information collected and tabulated in the statements referred to by Mr. Scaife, will be found of little practical value, and of no particular use for actual application.

What gave the subject its importance six years ago, was the alarming destruction of the rail-joints under the traffic, and the consequent danger to which life and property was exposed. When attention was directed to this state of affairs by the discussions which the subject brought forth, pains were taken at once to strengthen the joints by increasing the sizes and improving the shapes of the splice-bars, which very soon resulted in a general and well-marked improvement in the rail-joints on nearly all the better roads in the country. Many new devices were introduced and placed upon trial, but the angle bar type which largely prevailed six years ago, is still continued by preference in its improved form, and now gives such general satisfaction, that very few other devices have survived a temporary trial.

Not only have the angle bars themselves been increased in size for the sake of strength, but the constantly increasing rail-sections have naturally compelled larger dimensions in the angle bars, and since this increase is mostly in direction of the height, the strength and stiffness of the bars have consequently gained in double proportion. It is also conceded that by the gradual use of steel in the manufacture of angle bars, their wearing quality as well as strength has been considerably improved over the old iron bars, which, it is likewise conceded, were not always composed of the choicest metal which the market afforded.

By the lengthening of the angle bars, and the introduction of additional splice-bolts, it is generally believed, that the joints are made smoother and stronger.

Furthermore, by the introduction of heavier rails, the projecting ends of the rails themselves are strengthened and the depressions at the joints, which have been the principal causes of the breaking of the splice-bars heretofore, have been reduced to a very great extent.

The questions, with the solution of which this committee was charged, have thus practically solved themselves in actual practice and in a manner far more conclusive and satisfactory than could possibly be done by either theoretical investigations or experimental tests or both combined.

This committee finds itself, therefore, impressed with a feeling that its "occupation is gone," and with regets for its shortcomings, and apologies for the disappointments which it may have caused you, I, for one, would respectfully request that it be relieved from further duty.

Mr. Frans Engström: Being one of the surviving members, I beg to express my regret that, owing to the crippled condition of the committee, we were unable to complete the investigations which, as you have heard from Mr. Scaife's remarks, we had blocked out for us on a very large scale.

The question of joints is by no means a new one. I am nigh to believe it is older than this Committee; nay, even Shakespeare must have been bothered with it, or else why did he complain that "the time is out of joint?" It is not the least doubt to my mind but it was a sympathetic feeling for this committee that prompted him to continue:

> O, cursed spite,
> That I was ever born to set it right!

Judging from the age you might have considered us as a standing committee, but I can assure you we have not been standing all this while; that would perhaps have been too tiresome. In the beginning of our existence we sat down occasionally to work, we held a great number of well attended meetings and carried on quite an extensive correspondence, the result of which you will find in the two large volumes mentioned by Mr. Scaife.

It is from there I abstracted tables 1 and 2, which were to be considered only as preliminary work for the use of the committee; same remark also refers to table 3. Let me here again call your attention to the fact that the tables should not bear the date of this year, but show the status of things at the time the answers were received—that is 1886 to beginning of 1888.

Happily enough, however, the knotty problem of the breaking of rail-joints was not depending on this committee for solution.

Once the question was stirred up, the heretofore almost neglected joint received the attention which its importance deserved, and under the tender care now bestowed upon it, improved and grew stronger, not only in length, height, and thickness, but also in the fourth dimension, quality of material.

Such a treatment could not help having some influence on its somewhat loose morals. It became aware of the duties imposed upon it by the traffic and gradually quit its bad habit of snapping off, many a time apparently without the least provocation.

A glance at the tables shows that the angle bars most generally break at the centre, from top downward. I fail to see how this could cause any surprise. The top fibres already exposed to the severest punishment by being at the greatest distance from the neutral axis, are still more handicapped by the narrowing up of the section at this point; whereas, the bottom fibres, in addition to the advantage of a wider section, also are given more play-room for their elasticity in the lower flange before they could begin to break.

This is independent of whether you prefer to consider the bar broken by the two rail ends acting as cantilevers or by the upward bending moment which, if the joint ties should happen to be tamped a trifle more solid or higher than other ties, may be more dangerous.

One answer (from N. P. R.R.) ascribes the failure of the bars to an entirely different cause; the thermometrical range, and no doubt this has a great deal to do with it in a location where the thermometer without the least hesitation jumps from 115° above zero, in the shade, to 46° below zero; sometimes as much as 60° within six hours. Others lay the whole blame on the condition of road-bed, ballast, etc.

As we learn most from the mistakes of ourselves and others that come under our observation, I was under the impression that we were going to reap the richest harvest of information from those who were not satisfied. It will be seen, however, that table No. 1 gives pretty nearly as many suggestions as the other one, and it is amusing to note how several answers strained the meaning of "satisfied" by telling how they were going to change the bars. And, as far as my knowledge goes, the by far greatest improvements are made on roads which at that time were perfectly satisfied with the joint as it then existed.

Notwithstanding the great strides the last few years have made towards improving the rail-joint, and even with the backing of some 36,000 miles of the best railroads in the country, I am not convinced that we have yet reached perfection. It certainly does not seem very consistent to require the joints in a bridge to be so and so many per cent. stronger than the members joined, and on the other hand be fully satisfied if only the rail-joint comes up in the neighborhood of one-half the strength of the rail. I presume, however, the best we can do is to exercise our patience in waiting until some inventive genius gives us a joint fully as strong as the rail, which automatically keeps the bolts tightened up, if bolts be needed, and also is capable of restoring the continuity of the rail. But then there would be no need of a committee on rail-joints.

When "la grippe" visited our shores for the first time, the doctors were at a loss to know what it was or how to handle it, and had to content themselves with making prescriptions on a guess and in the meantime study its nature and compare notes on the remedies. Thus, when it made its second appearance, they were fully prepared to make short work of it. In a similar way it is to be hoped that next time the epidemic of breaking rail-joints comes upon us these fragmentary notes may add their mite to help the engineers to more successfully put on the gloves with it.

PRESIDENT HUNT: I would like to ask Mr. Engström, in what direction the changes of the shape of the section have been made to strengthen it as well as to get a better bearing?

MR. ENGSTRÖM: We all know the shape of the old-fashioned angle bar about one-half inch thick, with parallel sides and a very heavy base like this (illustrating). The material being nearly evenly divided between the two legs, the neutral axis is consequently very low. Now the tendency is always to remove the material from the centre of the web, if it so be called, and the base where it also well can be spared and increase the top and bottom (illustrating), thereby approaching a more rational section with wider bearing surfaces, and making it a good deal stronger. A few roads even go a step further and make the outside bar high enough to reach up within one inch or so below the top of the rail.

In this connection I would like to mention something that puzzled me when calculating the strength of the bars. I found a railroad using a splice-bar of very well proportioned section which they had to change to fit a heavier rail, later adopted. The height of the bar had to be increased, and this they accomplished by adding only a little new material and taking the balance from the top of the old section. At first sight it looked like a very good bar, but my figures did not come out that way, and the fact remains that the new bar, although the section was somewhat increased, has a smaller moment of resistance and consequently is not as strong as the original bar used with a ten-pound lighter rail.

PRES. HUNT: I might add here in line with the question of quality already referred to by the committee, that in the manufacture of these fish or angle bar joints since the failure of natural gas in the Pittsburg district, there is an increase in the number of steel bars over iron. In my own experience I find that at least 75 to 80 per cent. of all the orders we inspect are of steel. There are still some 20 to 25 per cent. of the angle bars being made of iron. Two years ago the proportion was not over 50 per cent., and two years before that I think the proportion was 5 per cent. of steel. I would like to ask Mr. Becker what his experience has been in the rapidity of the change in the use of steel joints over the iron joint?

Mr. Becker: The information that one gets by making inquiries is very varied. I am a member of an official committee which, among other things, is charged with an investigation of this very subject; and we have been trying to find out what proportion of steel angle bars is now being used. We have addressed the manufacturers of angle bars and they have sent us lists of the parties who have ordered steel angle bars, and then we have addressed the parties who were named in the circulars, and we got responses which I will mildly call "contradictory." The two do not "gee" very well. The fact of the matter is, even at the present time, although most everybody will say that at equal prices steel bars will be better and more economical in the long run, yet but a very small proportion of the whole are being used. The Pennsylvania railroad is not using any, except perhaps for experiments. Our lines have not used any yet. A number of the officers of the lines referred to by the manufacturers stated they had simply ordered some as samples for the purpose of making experiments; and others who had ordered them were not prepared to say they had any great superiority over iron, although they thought they had some. I am quite willing to express my own belief in that direction. But there is one thing quite certain, that the whole excitement regarding the dangerous condition of the track with regard to angle bars has practically died out. There is no longer any danger from that source. It was in this Society reported some seven years ago that on one five-mile division of our road there were some 1600 broken angle bars; to-day we have no reports of broken angle bars from any part of the road. There is no longer any necessity for discussing the subject. It is just like a cholera scare—it dies out when the disease has passed.

Mr. R. N. Clark: I would like to ask why is it that every one thinks these fish plates should be made of soft steel instead of good iron?

Mr. Becker: Because we find that in other shapes of equal dimensions the steel is stronger than iron, for compression or any other strain.

Mr. Clark: Is steel stronger against the jar and shock that takes place there?

MR. BECKER: It certainly must be, if the sample tests are to be taken. I am not prepared to say that steel, either in the manufacture of angle bars, I-beams or any kind of shape, would be as safe under a blow or under a falling body as iron would be, and in a measure that treatment is the one to which angle bars on a rough track are subjected. But under all other tests, for tension, for elongation, for elastic limit, bending or the ordinary tests to which metal for these uses is subjected, steel has shown itself superior, as we all know, and from that the general impression has gone abroad that steel would be preferable.

MR. LEWIS: I would like to ask, Mr. President, if Mr. Becker or yourself can give us an idea of the class of steel, that is, the physical properties, that is now being used in angle bars; that is, the ultimate strength, elastic limit, or at least some average of what is being used.

MR. BECKER: I do not remember now just exactly what is the prescription, but the specifications which were sent to these manufacturers, to which I have before alluded, gave a specification in detail as to the character of the steel. Ordinarily speaking, it would be the same class of steel that is used in bridge construction.

PRES. HUNT: I am at present inspecting rail joints for five different railroads. The steel is soft Bessemer steel, .10 to .15 carbon. It is steel of from 50,000 to 65,000 pounds tensile strength per square inch, an elastic limit of one-half the ultimate, an elongation of 20 per cent. minimum in eight inches and a reduction of area of at least 35 per cent. It is a lower requirement than for bridge materials. Some of the roads require 40 per cent. reduction of area.

I would like to state with reference to the problem of the use of steel to replace iron, that the best and most authoritative investigation that I know of in this matter was the very extended one by the Navy Department before they adopted the use of steel over iron in the construction of the new navy of our government. This commenced away back in 1881 and 1882. Mr. James Park, Jr., was one of the first steel manufacturers to urge the use of steel over iron. That must have been as far back as 1880.

For four or five years the Committees of the Navy Department and the Committees of Congress appointed to report upon the subject continued a very extensive examination of the subject. They investigated the tests that had been made abroad and found that under impact, as in the case of a vessel's bottom coming in contact with a ledge of sunken rock, the effect would be less appreciable on steel than on iron, that steel would stand punishment better, and was in every way a more suitable metal than iron for vessels.

In the same connection could be cited another place where steel is now rapidly replacing iron, and that is in bridge structures. It has been found that the softer grades of steel have stood the impact of trains running off the track and striking the web system of bridges in better shape than iron. That may depend considerably upon the quality of the iron, but in that connection I can state, I think, without controversion from the manufacturers, at least of the Pittsburg district, that almost every large iron and steel concern furnishing structural material has given the preference to steel, at the sacrifice of the considerable skill and careful attention that was necessary to keep up their iron product. That has been a very serious matter. To-day, with many sections, it is almost impossible to get them rolled in iron. The changes in the demand have occasioned the mills to change their rolls to make sections in steel rather than to make them in iron, and as the same sections of rolls will not fill out in iron in the same way as they do in steel, it has occasioned the sections to be defective in iron.

MR. BECKER: I would like to say in answer to Mr. Clark's inquiry, that the conclusions which railroad men have arrived at regarding the substitution of steel joints for iron joints have no doubt been largely induced by the use of steel rails as against iron rails, and they have been so thoroughly convinced, that there is not a skeptic in the country to day. I remember very well, that only as far back as about 1870, a steel rail was a great luxury. It cost in those days $130 per ton. Only the richest roads in the country could afford to buy steel rails. The prevailing track was laid with iron rails. The iron rail, under modern traffic, would last from six months to two or three years. I have known many and many a lot of iron rails that were laid in our tracks to be wholly and literally destroyed with six months' wear, the heads chafed off, bent cross-wise and worn down clear to the rail-web. Now, we are getting our steel rails at from $28 to $30 per ton. They last under the same kind of traffic which destroyed the iron rails a few years ago in six months, from twelve to fifteen years. They do not break so much, they wear out beautifully, and they are giving far better general satisfaction, and there is but reason to suppose in the same way we would get the same service out of steel rail joints which are nothing more nor less than a portion of the rail itself.

MR. DEMPSTER: In the paper just read Mr. Becker stated that it did seem that the quality of the material was a very important element. If after six years' experience they have got the material and they do not break any it certainly shows a superiority in the quality of the material. Now the question you raise, as I understand it, is that the physical properties of iron are not inferior to the physical properties of the steel.

MR. VICTOR WIERMAN: I would say, that our experience has been similar to Mr. Becker's.

As to breakage; we have experienced but little trouble from that source since the rail section has been increased and the angle bar proportionately made heavier.

We have tried a great many different styles of splices on the Pennsylvania Railroad during the past few years; some have had merit, more have not. But we always come back to the old pattern angle splice bar. It is a good joint, the best I have yet seen. It is not perfect by any means; I would like to have a better one; most railroad men would, but for all around work it stands the service better than any yet produced as far as I have seen.

In looking over the proceedings of the Engineer's Society of Philadelphia, for October, I see this subject was discussed at some length, and several patterns of rail joints submitted, all of them being strengthened angle splice bars, the additioual strength being obtained by getting greater depth with bolts under the rail (passed the drawings to the members). They look all right, but I have not seen them tested practically, so cannot pass judgment on them, and merely mention this to bring to the notice of the meeting the fact that the question is an important one, and is being discussed by other societies.

Pres. Hunt: There is some danger from corrosion too. Mr. Davison said he would liked to have been at this meeting to have stated that down at Chartiers they laid some rails in ashes a few years ago, and when taken up the webs of the rails were almost entirely corroded away. While ashes are not to be praised for ballast there is more or less corroding action in the ordinary material used for ballast.

Mr. Wierman: That is very true. In a tunnel in the Allegheny Mountains, some 4,000 feet long, where there was never any trouble with anything but water, you will be surprised to hear that 85-pound rails, the heaviest we use, will wear out on the bottom to a feather edge. They last only two years.

Mr. Becker: We are not so afraid of corrosion. Rails are not generally laid in ashes. We are willing to take our chances on corrosion, but it is the sudden breaks that happen on railroads which are to be guarded against.

Mr. S. L. Tone: I would like to hear the opinion of the Committee on the difference between a supported and a suspended joint; whether it is advisable to put a plate under the supported joint.

Mr. Becker: I will refer the gentleman to a paper which I read here some six years ago. If everything is in print here, as has been stated by the Secretary, that paper is accessible. I think I dwelt on the difference between suspended and supported joints, and also upon the so-called three-tie support as against the two-tie support. As to the plates under the tie I will say I do not believe much in tie plates. I know that is a very popular subject just now, and there are some very pretty pictures sent around by the parties engaged in the manufacture of these plates, but I have about made up my mind that I would not use a tie plate on the ordinary good size hard wood cross-tie. I believe that the tie plate might be of some service in cases where the tie is liable to wear out by the friction of the rail earlier than it would by internal decay. Where, for instance, the tie is wholly removed from the cause of decay, as on a bridge, and is liable to be scar by the action of the rail a plate may possibly induce a li longer life in a tie, but when the tie dies from rottenness I do think any tie plate will save it. On the contrary I am not s but that the plate will hasten it along.

There are other objections to tie plates. One is they are made of sufficient thickness, and I notice that they buckle twist. But even when you make them of a thickness to do good you then have to resort to a longer spike in order to get same adhesion of the rail on the cross-tie, for you lose the eff ive length of the spike to the extent of the thickness of your p That is about my idea of a tie plate.

MR. TONE: Would not a tie plate be of service in frogs?

MR. BECKER: What I said with reference to the ordinary base applies with greater intensity to the frog base, because frog has so much more bearing surface. You do not want plate, for it necessitates the cutting down into the surface of cross ties or switch-ties, as they are called. I think the ought to be of sufficient width and always is made of suffic width to get a good bearing without requiring plates under frogs.

On motion the Committee on "Rail Joints" was ordered charged, with the thanks of the Society, and the Secretary instructed to send copies of the report and discussion to parties who had so kindly supplied information to the C mittee.

Adjourned.

R. N. CLARK,
Secreta

MR. EDWARD B. TAYLOR (communicated): While the Comi tee has furnished us some very interesting statistical informat yet the attention which the subject received some six years when the Committee was appointed, has resulted in a material provement of the splice bars as well as the rails themselve that there is not the trouble at this time that there was in 188

BLE

TIES.
Kind.
....
White oak.
White & burr o
Oak, ash & ced
Oak.
White oak, 28 p S. yell'w p., 22 Chestnut, But'rnut, } 27 Heml'ck, Cedar, } 23
....
....
White oak in main line.
Cedar, white oa chestnut, hemlo
White oak, rock oak.
White oak, burr oak.
Yellow pine.
White oak.
.... White oak. White oak.
White oak.
Oak, chestnut, tam arack. Chestnut.
Chestnut.
....
....
Hemlock, oak.
Oak, tamarack an hemlock. Tamarack, cedar hemlock. ....
....
Hemlock, cedar.
Cedar, chestnut.
Sawn oak.

3LE I.

Ties.						
Kind.	Distance, center to center.		Material.	Is it satis-ıctory?	Changes suggested.	Result of Tests.
	At Joint.	Elsewhere.				
....	Inches. 20	Inches. 23	Iron	Per-ectly.	Now making them heavier.	Ult. strain, 49,227 lbs. El. limit, 33,128 " El. in 6", 8.3 per cent.
White oak.	17	23½	Iron	atis-ctory.	......	....
White & burr oak.	24	24	Wroug iron	Per-ectly.	......	At mills, Tensile,45,000 lbs., lowest allowed; guar'nt'd.
Oak, ash & cedar.	24	24	Iron	atis-ctory.	......	None.
Oak.	15½	21	Iron	atis-ctory.	......	None.
White oak, 28 pr ct. S. yell'w p., 22 " Chestnut, But'rnut, } 27 " Heml'ck, Cedar, } 23 "	15 B 14	24 24	Rolle iron	atis-ctory.	Make 4" to 6" longer, to provide 3" more tamping room.	None.
....	18	22	Steel	atis-ctory.	......	None.
....	16	23½	Iron Steel	atis-ctory.	......	Nothing very reliable.
White oak in main line.	19	24	Iron some s	atis-ctory.	......	....
Cedar, white oak, chestnut, hemlock.	3 per jt. 3000 p	...... er mile.	Iron	atis-ctory.	......	None.
White oak, rock oak.	18	21	Iron	atis-ctory.	......	None.
White oak, burr oak.	15	15	Iron	atis-ctory.	Make 36" long, to reach over 3 ties; greater cent'r str'ngth	None.
Yellow pine.	24	24	Iron	atis-ctory.	......	None.
White oak.	17	22	Wroug iron.	New t'sf'y	......	Ult. str., 46,850; el. limit, 26,460 lbs.; el. 15 pr. c. Reduction, 21 pr. c.
....	15	23½	Iron	New t'sf'y	......	....
White oak.	16	20	Iron.	"	......	....
White oak.	16	20	Iron	atis-tory.	......	No tests.
White oak.	17	22	Wroug iron.	far. y sat-ct'ry	......	El. limit, 26,125; tens'n, 53,100 lbs.; el. in 2" 18 pr. ct.; red. of area, 18 p. c.
ak, chestnut, tamarack.	16	16	Iron	t is.	None.	Up to P. R. R. standard.
Chestnut.	20	23	Iron; fe steel	t is.	Make of steel.	Tensile, 50,000 lbs., Per specification.
Chestnut.	22½	22½	Iron	es.	No suggestion.	None.
....	19	24	Iron	es.	......	....
....	24	24	Iron	es.	......	None.
Hemlock, oak.	{ A 24 { B 16	24 24	Iron	es.	......	None.
ak, tamarack and hemlock.	15	24	Iron	rly o.	Better material.	None.
amarack, cedar & hemlock.	15	24	Iron	"	"	None.
....	24	24	Iron	"	......	None.
....	......	......	Anneal steel	od ults.	......	....
Hemlock, cedar.	17 ties t	o 30'	Iron	est c'v'd	......	None.
Cedar, chestnut.	12	23	Iron	ual any.	......	None.
Sawn oak.	17	24	Steel	est ed.	Samson angles exclusiv'ly	None.

TA

ace d and t.	Size
es.	6″ x 7″
5	7″ x 8″
5	6″ to 7″ x
½	6″ x 8″
5	6″ x
7	6½″ x 9
	6″ x 8″
	6½″ x 8
5	7″ face
5	7″ x 7″ 7″ x 7″ 6″ x 8″
	7″ x 9″
	6″ x 8″
	6½″ x 8′
	......
½	7″ x 8″
5 5	6″ x 8″
7	8′ &
5 5	6″ x 8″ 7″ x 9″
½ ½–7	6″ min. fa
	6″ x
5 5	6″ x 8″ a x 8
5	7″ x 7″
5 6 5	6″ x 8″ m
5	.....

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† Do a

TABLE

COMMITTEE ON RAIL-JOINTS.

ace l and t.	Size.	How?	Yrs. in use.	Is it satisfactory?	Changes suggested.	Result of Tests.
es.	6″ x 7″ x 8′	les.	8	No.	Prefer angles.	......
5	7″ x 8″ x 8′ 6″	...	4	No; 50 & 54 too light.	......	None.
5	6″ to 7″ x 8′ x 8′	extnd'g ; some- down.	2½	No.	......	None.
½	6″ x 8″ x 8′	m top	6	No.	Better quality of material.	......
	6″ x 8″	m top	9	No; but best yet tried.	Higher&heav'r outside angle of mild steel.	Stood 50,000 lbs. Bend 90° without fract.
7	6½″ x 9″ x 8′	oint.	10 2½	......	Should be of steel, heavier and longer.	No exact.
	6″ x 8″ x 8′	.	3½	Not entirely.	None.	None.
	6½″ x 8″ x 8′	om top	3	No.	Try "Fisher."	Quality of iron found good.
5	7″ face x 8′	to in- tre.	4 to 6	No.	*	......
	7″ x 7″ x 8′ {7″ x 7″ x 8′ {6″ x 8″ x 8′	betw'n ce enter.	5	No.	......	None.
	7″ x 9″ x 8′ 6″	n top	6	No.	Angle splice over tie.	None.
	6″ x 8″ x 8′	p c'nt'r hole oint. set.	10 2	A No. B Very fair.	12″ immovable rail; 2 joints of ½ expansion over ties.	Careful observation of wear and results, only.
	6½″ x 8″ x 8′	n top	1	......	......	None.
	......		8	......	......	None.
½	7″ x 8″ x 6′ 6″	d. bolt- S often plice.	4–5 6–7	......	†	None.
5 5	6″ x 8″ x 9′	le, at nt.	6–13 5	......	......	None.
7	8′ & 9′	center n.	4 12	......	Put jt. over tie.	......
5 5	6″ x 8″ x 8′ 7″ x 9″ x 8′	enter, con- bed.	O.,8 N.,2	It is not.	Heavier, long'r and reinforced.	None.
½ —7	6″ min. face x 8′		...	......	Joint over tie.	None.
	6″ x 8″	top	4	......	For susp. joint, more sec. at cntr Joint over tie.	......
5 5	6″ x 8″ and up x 8′	top	8	......	Favor "Fisher" or joint over 3 ties.	None.
5	7″ x 7″ x 8′ 6″	m.	4 1	......	Prefer 2 angles.	......
5 6 5	6″ x 8″ min. x 8′	t.	Since flood.	Not at all.	Extra heavy Samson.	"Not much."
5	......		Just adopt.	......	......	......

† Do away with

Lehigh Valley R. R.	67 lb.	6.79	1.63	18.04	47.88	6.26	0.96	5.77	27.12	92	32	57

45,000 lbs.
25,000 "
ce betwee

LE III.

Lehigh Valley R. R. ....67 lb.	6.79	1.63	18.04	47.88	6.26	0.96	5.77	27.12	92	32	57
Maine Central R. R. ....67 lb.	6.88	1.60	17.68	44.26	5.11	0.85	3.71	8.76	74	21	20
Michigan Central R. R. ....65 lb.	6.44	1.60	16.49	37.63	5.23	0.90	4.21	29.72 16.51	81	26	79 44
Mobile and Ohio R. R. ....60 lb.	5.89	1.49	13.16	......	5.32	0.88	4.07	......	90	31	...
N. Y. Central and Hudson River R. R. ....80 lb.	7.78	1.85	26.51	55.69	7.48	1.15	9.93	Support'd joint.	96	38	...
N. Y. Central and Hudson River R. R. ....65 lb.	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Savannah, Florida and Western Ry. ....60 lb.	5.89	1.53	13.81	48.61	4.97	0.93	4.27	10.86	84	31	22
Savannah, Florida and Western Ry. ....50 lb.	4.80	1.35	8.74	34.52	3.62	0.76	2.09	5.97	75	24	17
St. Paul, Minneapolis and Manitoba Ry. ....75 lb.	7.40	1.67	20.53	49.88	4.70	0.87	3.53	Support'd joint.	64	17	...
St. Paul, Minneapolis and Manitoba Ry. ....56 lb.	5.66	1.54	13.40	31.60	4.32	0.87	3.24	7.94	76	24	25
Wabash, St. Louis and Pacific Ry. ....63 lb.	6.22	1.58	15.51	47.50	5.29	0.90	4.26	34.50	85	27	73
Wabash, St. Louis and Pacific Ry. ....59 lb.	5.87	1.51	13.30	40.91	4.83	0.88	3.79	18.35	83	29	45
Wheeling and Lake Erie R. R. ....56 lb.	5.49	1.38	10.39	33.92	4.31	0.78	2.63	7.55	79	25	22

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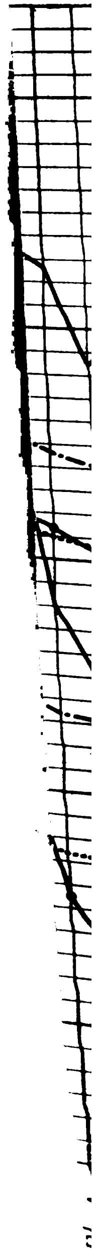

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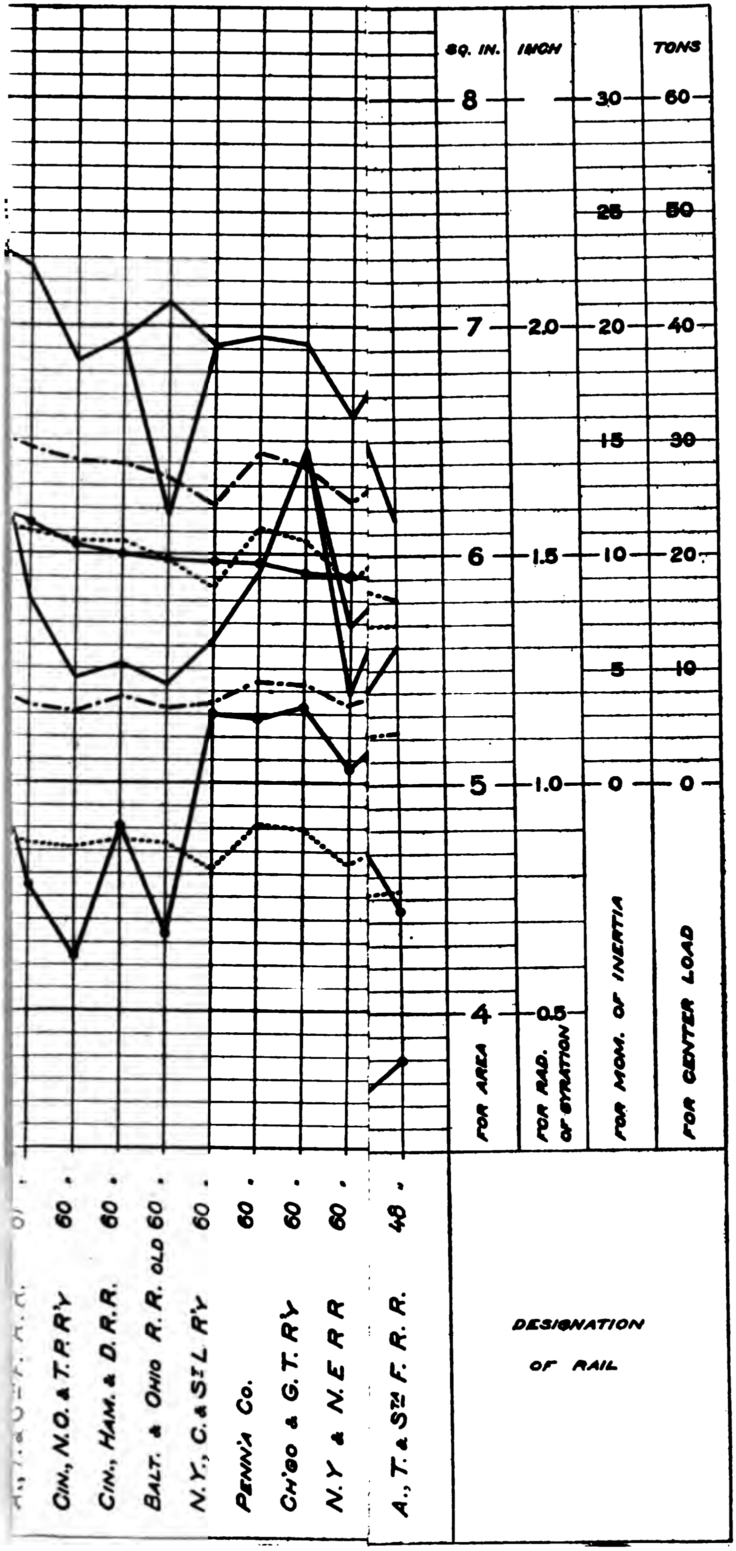
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PENN'A CO. 60
CH'GO & G.T. R'Y 60
N.Y. & N.E. R R 60
A., T. & STA F. R.R. 48
DESIGNATION OF RAIL

Personally I believe that the perfect joint will be not only a splice, but a bridge, and I think that the new Fisher joint approaches more nearly the perfect mechanical joint than the angle splice, although there is a prejudice against this joint on the part of many of the engineers and road masters, because it is not well adapted for switch connections, and requires the adzing of the joint ties, or, if this is not done, the sinking of the joint ties below the general level of the other ties, causing the water to collect in and about the joint ties and theoretically making it a little more difficult to maintain the track in good surface; although, I believe, the experience has been that even with the old Fisher joint, which was simply a bridge, the surface of the track was kept in better condition than where angle bars were used.

For the past two or three years I have not given as much attention to maintenance of track matters as formerly, but understand that even with the largely increased size of the splice bars, quite a number of them are found cracked after they have been in service for a year or two.

MEETING OF THE CHEMICAL SECTION OF THE ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

JUNE 28TH, 1892.

The Chemical Section met at the Society's rooms, Fifth Street, Pittsburg. Prof. John W. Langley, chairman, being absent, Mr. James O. Handy was chosen chairman *pro tem.* Mr. James Camp acted as temporary secretary. Fourteen members were present.

Mr. R. B. Carnahan read an interesting paper on "The Analysis of Tin Plate." He described a simple and rapidly executed scheme for determining tin, lead, iron and manganese in tin and terne plate. He also described a method for tin in solder.

His paper was discussed by Messrs. Williams, Camp, Handy and others.

The discussion then turned upon alloys of tin. Mr. James

Handy described the analysis of Babbitt's metal (the separation of tin from antimony). Mr. George Marsh described the assay of some Mexican tin for gold. Mr. Voorhees described the analysis of phosphor-bronze as practiced in the Pennsylvania Railroad Laboratory. Mr. Handy spoke of a device used in standardizing iodine solution. Several points concerning the determination of carbon by combustion were brought out.

The section adjourned until the fourth Tuesday in September.

A communication from Mr. Jos. M. Wilson, of Mingo Junction, described a method of determining phosphorus in ferromanganese *without evaporation* to dryness. It was:

"Dissolve 2 grammes of ferro. in 40 c.c. conc. HNO_3, boil down one-half, dilute to 60 c.c., add permanganate in excess, and proceed as described in Handy's method."—*Proceedings Chem. Sect.*, March, 1892.

Mr. Carnahan then read his paper:

THE ANALYSIS OF TIN PLATE FOR TIN, LEAD, IRON AND MANGANESE.

The determination of tin in tin or terne plate by the stannic oxide method (oxidizing by nitric acid) is very unsatisfactory.

The reasons are: first, that it is difficult to completely separate the stannic oxide; and, second, if this is accomplished, the precipitate is sure to carry a considerable quantity of ferric oxide and small quantities of lead and manganese oxides.

The stannic oxide may, of course, be purified by fusion, but this is slow and troublesome.

The following volumetric method, depending on well-known reactions, has given very satisfactory results:

Dissolve 5 grammes of tin or terne plate in 100 c.c. HCl, 1.1 specific gravity, in a 500 c.c. graduated flask, with exclusion of air.

When dissolved, cool and fill up to the mark. Transfer 50 c.c. to a beaker, and after adding starch paste, titrate the tin with standard iodine solution.

A convenient strength of iodine is made by dissolving 5.38 grammes of pure iodine in strong potassium iodide solution and diluting to 1 litre.

For the iron determination, add mercuric chloride in excess to 50 c.c. of the tin plate solution, and titrate the iron with standard bichromate.

The determination of manganese is quite important, since it shows whether *iron* or *steel* has been tinned.

The color method as ordinarily used fails utterly in tin plate, but with the following modifications, due to a suggestion of Prof. F. C. Phillips, it gives good results.

Treat 4 grammes tin plate, cut into small pieces, with hot dilute sulphuric acid for about fifteen minutes.

When the iron has dissolved, leaving the layers of tin and lead, add a little zinc and let stand for about two minutes. Filter and dilute to 20 c.c.

Take one-half of this filtrate, add 5 c.c. nitric acid of 1.2 specific gravity, and treat in the ordinary way with lead peroxide.

The lead in tin plate is best determined as sulphate after first separating the tin by nitric acid. However, for ordinary work, it is sufficiently accurate to take lead by difference, allowing .25 per cent. for phosphorus, carbon, sulphur, silicon, etc., in addition to the tin, iron and manganese previously determined.

In order to test the accuracy of the iodine method for tin, a weighed quantity of pure tin, together with about forty times as much iron, was dissolved and the tin titrated.

The result was as follows:

Amount taken, .1255 grammes tin.

Amount found, .1266 grammes tin.

The following are a few analyses (p. 184) that I have made of British terne plate used for roofing.

The iodine method may be used for determining tin in all alloys which contain no metals that affect iodine.

However, when the percentage of tin exceeds 10 per cent., as in the case of solder, the following method, although not quite so simple or rapid, is somewhat more accurate.

In principle, the scheme is simply a reversion of the well known stannous chloride titration method for iron.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Tin	1.58	2.08	2.40	3.37	1.60	2.54	1.97	1.96	2.56
Lead	7.97*	7.13*	8.89	11.98	2.48*	7.48*	8.12*	7.09	10.23*
Iron	89.84	90.23	88.10	84.18	95.31	89.35	89.29	90.55	86.64
Manganese	.36	.31	.31	.35	.36	.38	.37	.32	.32
Carbon Phosphorus Sulphur Silicon, etc.	.25	.25	.25	.25	.25	.25	.25	.25	.25
	100.00	100.00	99.95	100.13	100.00	100.00	100.00	100.17	100.00

* By difference.

Dissolve 5 grammes of the tin alloy in strong HCl in a 500 c.c. graduated flask as in the case of tin plate. After diluting to the mark, fill a 50 c.c. burette with the solution. Transfer 10 c.c. of a standard ferric chloride solution (10 grammes iron in 1 litre) to a 4 ounce flask and heat to boiling. While boiling, run the tin alloy solution cautiously into the ferric chloride until the yellow color disappears. Cool and determine the excess of stannous chloride with standard iodine solution.

The reaction is:

$$Fe_2Cl_6 + SnCl_2 = 2FeCl_2 + SnCl_4.$$

Knowing, by the equation, that 56 parts iron equal 59 parts tin, and adding to this the slight amount of tin by iodine titration, and at the same time knowing the volume of tin alloy solution used, we can readily calculate the tin.

This method was used, and gave satisfaction, in the analysis of a sample of solder containing 47.8 per cent. tin in the presence of over 50 per cent. lead.

DISCUSSION.

MR. HANDY: Mr. Carnahan's method is simple and ingenious, and I am sure that we are all obliged to him for describing it. Although the analysis of tinned-plate is not very often required, the separations of tin, iron, lead, and manganese from each other are frequently necessary.

I have already mentioned that tin cannot be separated from iron in tinned-plate by treatment with HNO_3 and evaporation to dryness. When the residue is boiled with dilute nitric acid, the metastannic acid is redissolved. It seems to be soluble in an acid solution of ferric nitrate.

I have separated the tin by evaporating the nitric solution of the tinned-plate with sulphuric acid, 20 c.c. per gramme of "plate." When the solution gives off fumes of SO_3, cool, add water, boil up, and filter off silica, if present. Dilute the filtrate to 300 c.c. warm, and separate the tin by sulphuretted hydrogen.

Mr. Carnahan's analyses of terne plate show the ratio of tin to lead in the coating to be from $\frac{1}{3.55}$ to $\frac{1}{3.70}$, and the material coated to be soft steel. Naturally, the percentage of tin in tinned-plate varies with the thickness of the plate. What gauge were the terne plates which you analyzed, Mr. Carnahan?

Mr. Carnahan: I do not know the gauge; they were the ordinary roofing plates.

Mr. Handy thought that the discussion might be profitably extended to other compounds and alloys of tin, and gave a brief description of a method for separating tin from antimony in Babbitt's Metal.

ANALYSIS OF BABBITT'S METAL.

(*Method of E. M. Bruce, modified.*)

Five grammes of drillings in an 8-oz. beaker are treated with 30 c.c. of HNO_3, 1.2 sp. gr., and heated till decomposition is complete and the free acid nearly all evaporated. When about 5 c.c. of solution remain, add 15 c.c. of water, and then add conc. NaOH solution till nearly neutral; 50 c.c. of *sodium sulphide* solution are then added, the mixture well stirred, then boiled gently for half an hour. The solution then contains the tin and antimony. The precipitate, which contain the sulphides of lead and copper, is filtered on a 9 c.m. Swedish filter, and washed thoroughly with water containing 1 per cent. of the above sodium sulphide solution. The filtrate is received in a 300 c.c. beaker.

TIN AND ANTIMONY.

The filtrate is diluted to 200 c.c., and boiled. Crystals of oxalic

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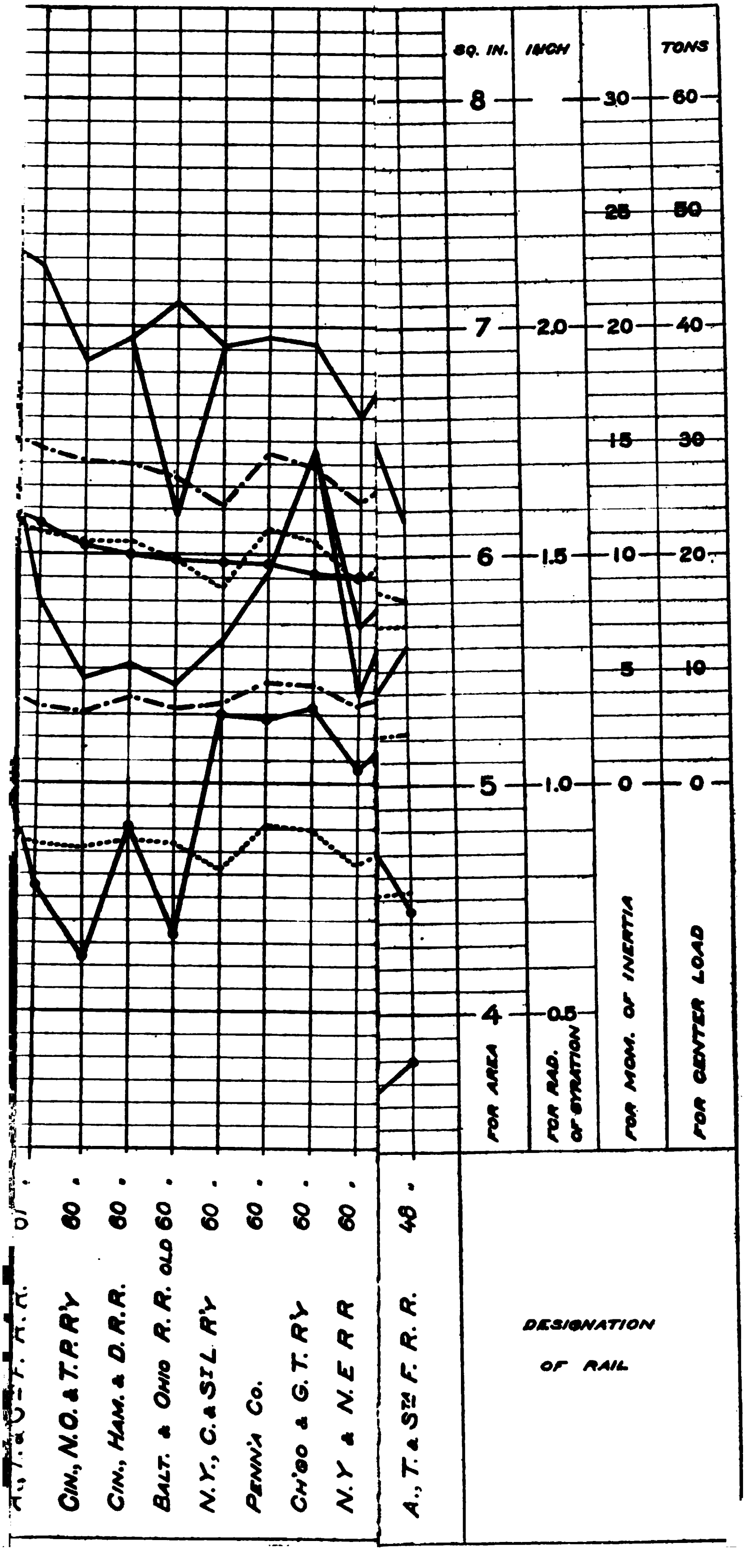
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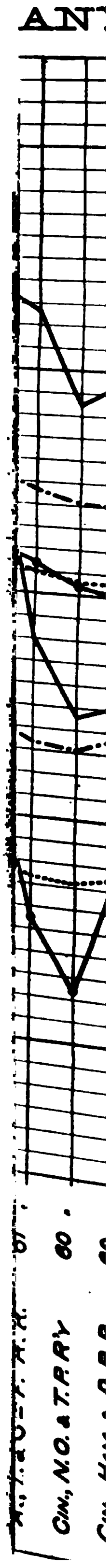
DESIGNATION OF RAIL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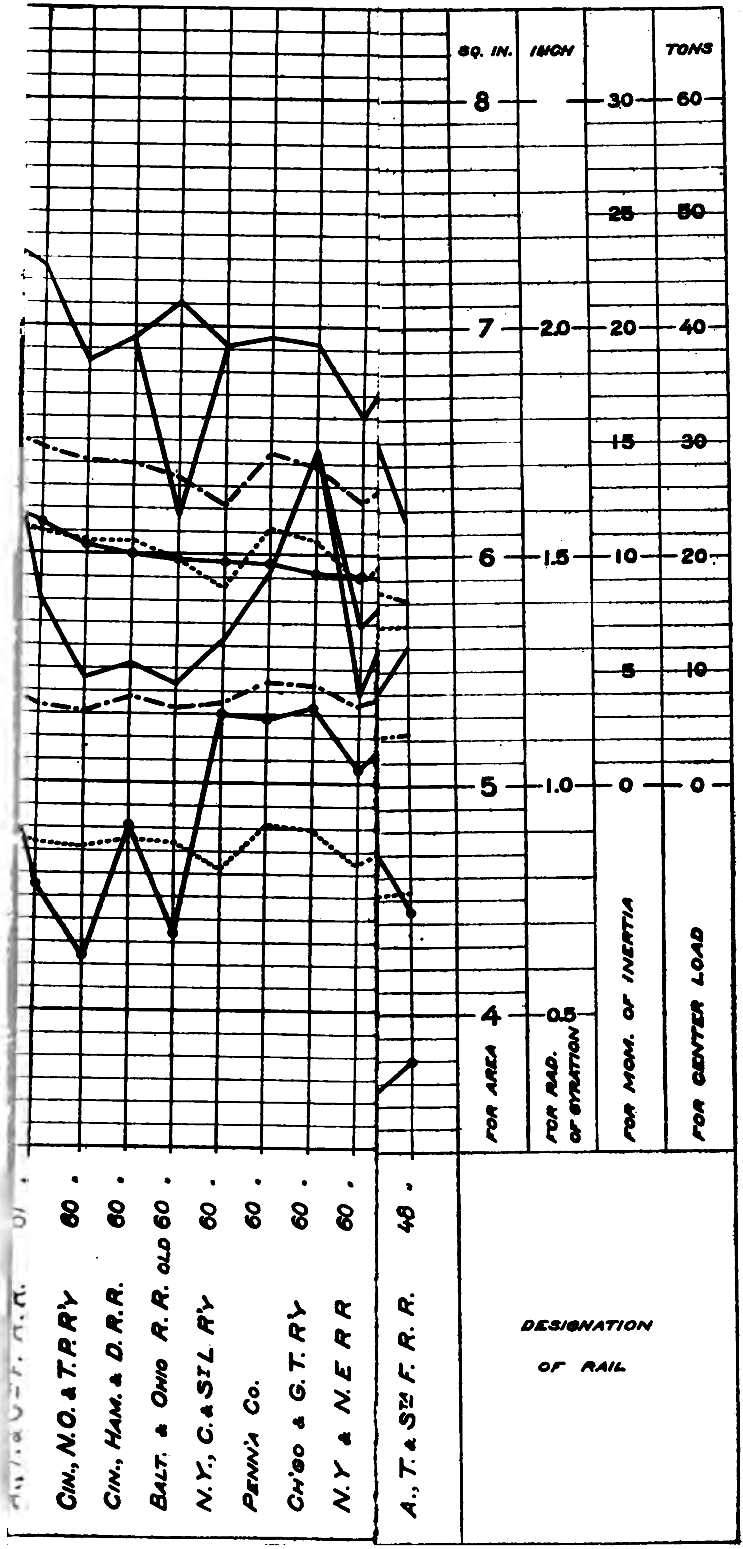
COMMITTEE ON RAIL-JOINTS.



made the first President, and a committee of five were appointed to propose a plan of the bridge and have it submitted at the next meeting of the Board.

The gentlemen comprising this committee were J. K. Moorhead, John Tassey, O. Metcalf, William Holmes and John Grier. The Committee did not take much time to make their report, for on the 7th day of January, 1837, at a meeting of the Board, a plan was submitted which was satisfactory to the Managers, and upon which proposals for the erection of the bridge were received until the 2d day of March following.

The plan adopted was that of the old covered bridge, familiar to all residents of Allegheny County, which was removed in 1890 to make way for the present steel truss bridge.

Out of a number of bidders who submitted proposals, the committee allotted the stone work to Pagan, Alston & Co., for about $37,000, and the superstructure to Le Baron & Lothrop for about $33,000. With the approaches the cost of the bridge originally was about $80,000.

The writer has searched for the drawings and models submitted at this bridge letting, and deplores to note his failure to discover any, as no doubt a review of them would be highly interesting at this date; nor has he been able to obtain any information relative to the contractors.

Of course an engineer was employed to give the necessary lines and levels, and to supervise the construction of the bridge, but no mention of one occurs in any of the reports extant to-day. No doubt, even at that early date, the engineer had developed the tendency to drop out of sight and hearing involuntarily or voluntarily upon the completion of the structure, such as his younger brothers possess. Whoever he was, he did his work very carefully, as the accuracy of the alignment and elevations was well established during the taking down of the old bridge.

From the inferior character of the masonry work, and the excellent nature of the geodetical work, it would appear that the engineer was merely employed to give lines and levels to the contractors. It is to be hoped that in the discussion of this paper, some light will be brought on these matters, so that the Society will be in possession of a complete history of the structure, at least from an engineering point of view.

In March, 1837, the contracts were signed. About $50,000 of the capital stock of the company had been subscribed for, the managers expecting to secure more subscriptions as the work advanced.

Before the contractors had made any progress in the erection of the bridge, the general suspension of specie payments by the banks, created so much alarm throughout the country, that nearly all improvements were suspended and the prospect of the company completing the work they had undertaken became very doubtful indeed; still, the Board not willing to relinquish the undertaking, instructed the contractors to go on and put down the foundations of the abutments and piers, so as to be able to resume the work early in the following year. An attempt was made in the month of August, 1837, to put down the southern abutment, but the contractors were driven from it by the city authorities, with whom the company had gotten into dissension. What the trouble was, the writer has not been able to ascertain definitely, but believes that it was caused by a fear on the part of the city that the construction of the bridge would unduly contract the river and increase the flood heights. The great flood of 1832, only five years previous to that time, no doubt had aroused the apprehensions of the citizens. In the early part of 1837, the Pittsburg Councils passed an ordinance granting the Pittsburg and Allegheny Bridge Company the privilege to construct an abutment at the foot of Hand Street; but on October 29, 1838, the ordinance was repealed, and another one passed which gave the Bridge Company the necessary authority, provided that the bridge would not decrease the waterway to a less area than the Pennsylvania aqueduct or any of the bridges already erected. During the controversy between the city and company, the masonry contractors put down the foundations of the northern abutment and piers. The first year, 1837, was thus consumed and but a small portion of the work done.

During the spring and summer of 1838 very little was done. The difficulties in regard to money matters were yet great and the trouble with the city not yet over. In the fall of this year an attempt was made to work at the Pittsburg abutment, but the authorities still resisted. Moreover, the company did not succeed in collecting instalments due from the subscribers, so that nothing whatever was done in the way of construction during 1838. The trouble with the city was adjusted during the latter part of the year. In the early part of 1839 the company gave up further attempts to obtain money from the sale of stock, and borrowed the remaining necessary amount, about $20,000, on the individual notes of the members of the company.

In 1839, therefore, the work of construction proceeded in good shape. In the spring of 1840 the company began to collect tolls, and in June of that year the bridge was taken off the hands of the contractors.

In addition to the construction of the bridge proper, the company had to pay one-half the cost of the bridge across the canal where the West Pennsylvania Railroad now crosses Anderson Street, and had to pave the street between the canal and the bridge.

From the time of its erection to the time of its demolition, the bridge passed through the rather uneventful career to which bridges are subjected. The Allegheny River, with its ice gorges and floods, on several occasions did considerable damage. In 1841 the Allegheny abutment was damaged to the extent of $1200 or $1300. In 1857 the first pier from the Allegheny side was so badly injured that it was necessary to suspend traffic. The cost of the repairs amounted to about $6000. It so happened that the company had insured the piers against damage from ice floods and so forth during the previous year. The insurance company, however, refused to pay the policy and the bridge company was unable to collect. The records do not give any reasons. This is the first time that bridge piers were insured, as far as the knowledge of the writer goes, and probably the experience of the Hand Street Company furnished the reason that other companies do not insure theirs.

The type of bridge selected by the managers of the Pittsburg and Allegheny Bridge Company in 1837 was of the latest approved form at that date. This was the Burr type of combined arches and trusses. Beginning on the Pittsburg side, the spans were: one span of 190 feet and four spans of 200 feet in the clear of masonry.

There were four piers and two abutments. The piers were 9 feet wide on top and 35 feet long; they had semi-circular ends, thus making the total length of the piers 44 feet measured along the centre line. The batter was 1 to 12. Skewbacks for the arches extended 4 feet down from the top of the piers, and were constructed on each side of the piers from edge to edge of the outer arches. Under the sidewalks, which were supported by cantilever beams, the skewbacks, of course, were not required. The foundations of all the masonry in the river were timber platforms. The price paid for the masonry was about $7 per cubic yard, which appears like a high price, as the work done was not such as would pass inspection at the present day. The face stones were of fair size and appeared all right on the exterior, but the backing stones were generally spalls set in ordinary lime mortar, which at the time of reconstruction had lost its strength, and was practically nothing but sand and earth. The stone used was of the Freeport sandstone variety, being somewhat soft and liable to be eroded by the river. On account of these conditions the piers, at the time of reconstruction, looked rather doubtful, but in reality were not so bad as they appeared. The abutments seemed a great deal better than the piers, but this was due to the fact that they were not subjected to the action of the water and ice.

When the Pittsburg and Western Railway was constructed, it was necessary for it to pass under the Allegheny approach. The opening made was 12 feet wide and 16 feet high, the southern side of this opening being placed 20 feet from the face of the abutment. The roadway was supported on buckle plates and iron beams, which were protected from corrosion, underneath, by galvanized iron plates.

Of the old superstructure, much could be said of interest, but the writer will desist from increasing the length of this paper unduly, and abstain from going into the details of the condition of the art of bridge building at the time of the construction of this bridge. The writer believes it was a structure which reflected great credit on the designer. In those days the construction of a bridge of 200 feet spans, subjected to heavy traffic, was a tremendous undertaking. The Hand Street bridge, indeed, was one of those structures which, in the first half of the present century, made the engineers of the United States deservedly celebrated throughout the world. The system of bridge construction advanced by Theodore Burr was the best known at that time, and the company displayed good judgment in its selection. The span here required had not been exceeded in the United States, except by the Schuylkill River bridge, Philadelphia, built by Wernwag, and by one across the Merrimac River. The Burr bridge was formerly used more than any. A description of this style of bridge will not here be entered upon, as such may be found in Trautwine and in Mahan's *Civil Engineering*. The great objection to it is, that a truss and arch cannot be made to act in concert, it being impossible to determine what part of a load is carried by each. The Hand Street bridge had three lines of arches, each being composed of two sticks and embracing the truss between its parts. There were two roadways, each 11 feet wide in the clear, and two sidewalks, each 7 feet wide. The middle arch timbers were twice the size of the outer arches, but the centre truss was of the same dimensions as the outer ones. The total height of truss was 14½ feet, the top of arches at the centre being flush with the top of truss. The radius of the arches was about 330 feet; actual span, 200 feet; rise, 15½ feet; depth of arch timbers, 2 feet and 4 inches; thickness of arch timbers, 7 inches. The total width of bridge outside to outside of arches was about 28½ feet. The clear height from floor to lateral bracing was 11 feet. As in all Burr trusses, the vertical tension members extended below the bottom chords; in this case about 18 inches. Particular mention is made of this as difficulty arose during the reconstruction of the bridge as to what constituted the lowest part of the bridge. This will be touched upon later on. The trusses had eighteen panels of about 11 feet each. See extreme left of Plate VI.

The bridge was covered on all sides and top, and offered great resistance to the wind. The top lateral system was on the Howe plan, the diagonals being of 6 inches by 6 inches white pine timber, and the transverse members of round iron rods. It could not transmit any wind strains to the piers, but could only transfer them to the arches and floor timbers through the stiffness of the various joints and connections. The wind strains were mainly taken care of and transferred to the piers by the rigidity of the arch timbers. These were considerably out of line for some years previous to the taking down of the structure, such condition being caused, no doubt, by the action of the wind and wearing of joints and bearings. A couple of the spans were considerably warped, and when the covering was taken off and the position of the trusses clearly exposed to view, a large number of people were fearful of instant collapse, and withdrew their patronage to neighboring rival bridges. The material used in the old structure was of the best clear white pine, such as is impossible to obtain now-a-days. When the bridge was taken down, no signs of rot were discovered in the main timbers, except in a few of the arch timbers below the floor upon which moist dust and dirt had been allowed to accumulate. The wood appeared to have lost its elasticity; it was very brittle.

I have so far confined your attention to the history and condition of the old bridge, and I will now set forth, as much as is in my power, such matters as may be of interest relating to the taking down of the old structure and erection of the steel bridge now in its place.

During the summer months of 1889, the majority of the capital stock of the Pittsburg and Allegheny Bridge Company was secured in the interest of the Pleasant Valley Electric Street Railway Company. The control was obtained by this Company for the purpose of replacing the old structure with one that would be adapted to rapid transit purposes, none of the existing bridges being able to offer the required facilities. The writer believes this is the first instance in which a street railway company in the United States has constructed a bridge of the size required to cross the Allegheny River. The railway company, of course, was only enabled to do this through the prospective increased earnings of its lines by the adoption of electric power. No street railway operated by horse-power could ever have been able to carry out such an undertaking.

The requirements of the company were:

1. The new structure to be wide enough for four lines of track,—two for rapid and two for slow traffic,—and to be supplied with two sidewalks each 10 feet wide.

2. The old masonry to be used.

3. The old structure to be used and traffic maintained during the erection of the new bridge.

4. The structure to be designed on strictly economical principles, no ornament of any kind to be used.

In addition to the above, specifications governing the main features of the new bridge were issued by the company. The principal requirements under these specifications, outside of those ordinarily called for, were:

1. The bridge to consist of four through spans of 206 feet and one of 194 feet, measured from centre of end pins. The clear head room between the floor and lowest point of transverse struts to be 20 feet.

2. The roadway to be designed to carry four car tracks of 5 feet 2½ inches gauge on eight iron or steel stringers. The clear width between trusses to be the greatest permissible on the old piers.

3. The roadway floor to consist of a bottom course of 3 x 10 inch yellow pine laid transversely and spiked to 5 x 8 inch pieces of yellow pine, placed on top of the stringers and bolted thereto, and a top layer of 3 x 6 inch white oak timber, surfaced on top, to be securely spiked to the lower course.

4. The sidewalk to be supported by three iron or steel stringers on which shall be bolted 3 x 6 inch spiking pieces of yellow pine, and to which the lower course of 2 x 8 inch yellow pine boards laid transversely were to be spiked. A second course of 1 x 6 inch white pine flooring, surfaced on the upper side and forming a wearing surface to be securely spiked on the 2 inch course of yellow pine.

5. *Loads.*—The roadway floor to be capable of sustaining a load on each or all tracks of 12 tons on two pairs of wheels spaced 10 feet apart, or a uniform load of 100 pounds per square foot. The sidewalk floor to be capable of sustaining a load of 80 pounds per square foot.

The trusses to be figured to carry a load of 60 pounds per square foot on the sidewalk and 80 pounds per square foot on the roadway, or a total of 4000 pounds per lineal foot of bridge.

The top laterals to be proportioned to sustain a lateral force of 125 pounds per lineal foot of bridge and the bottom laterals of 250 pounds. All treated as a moving load.

6. *Unit Strains.*—The following to be the allowable strains per square inch in tension :

	Pounds.
Steel bottom chord and main diagonals, . . .	15,000
Steel vertical suspenders, ...	10,000
Iron counters, ...	10,000
Iron lateral rods, ...	15,000
Steel bottom flanges of floor beams and stringers (net section), ...	12,500

In compression, the following strains per square inch to be used :

For steel members whose length does not exceed 16 diameters, ...	12,500

For longer members, $\dfrac{12{,}500}{1 + .004\,\dfrac{l^2}{r^2}}$ for members with two square bearings.

$\dfrac{12{,}500}{1 + .006\,\dfrac{l^2}{r^2}}$ for members with one pin and one square bearing.

$\dfrac{12{,}500}{1 + .008\,\dfrac{l^2}{r^2}}$ for members with two pin bearings.

A number of bridge companies were invited to compete, and in the early part of October, 1889, a number of plans were submitted. The proposition of the Iron City Bridge Company was considered the most favorable and the contract was awarded to it about October 15th. A few days prior to this the firm of engineers of which the writer is a member were appointed engineers for the company. Before the contract with the Iron City Bridge Company was entered into in writing, a number of changes had to be made in the lengths of spans and in the specifications.

The south-bound track of the electric railway at the Pittsburg end of the bridge turns into Duquesne way and in order to obtain a curve of 50 foot radius, it was found that the Pittsburg span would have to be shortened 40 feet and a new abutment constructed.

The Pittsburg and Western Railroad at this time expressed their dissatisfaction with the small passage way they had under the Allegheny approach and desired to negotiate with the Bridge Company for a wider opening and greater clearance. After a number of meetings, the matter was arranged satisfactorily. The Bridge Company agreed to construct a new pier 80 feet distant from the northern wall of the Pittsburg and Western passageway, to remove the old abutment and to build a viaduct of five spans of 16 feet each. By the arrangement the railway company obtained space for five tracks and sufficient head room to raise their tracks about 1 foot, a very important desideratum for this company, whose tracks at this place are subject to overflow. In the agreement between the two companies the probability of Anderson Street being raised was taken into consideration, and when this shall be done the railway can be further benefited by obtaining room for another track, by the removal of the present abutment, and can obtain an additional foot in height. The grade on the viaduct was arranged so that these changes can be made at any time in the future without detriment to the Bridge Company.

Pending the adjustment of these matters with the Pittsburg and Western Railway, we took advantage of the opportunity thus offered to change several items in the specifications with which we did not concur, and made a survey and profile of the bridge site, which had not been made before. In making the alterations we were restricted to the same weight of bridge that had been contracted for.

The changes were as follows:

1. The roadway to be supplied with twelve lines of stringers, instead of eight, as originally provided, a line of stringers in the centre of each track being necessary to provide for turning out of wagons and to safely carry teams.

2. The spiking pieces on top of stringers were done away with and the bottom layer of 3-inch planks were directly fastened to the tops of the stringers. Two layers of 3-inch oak plank separated by 1-inch oak boards were used, instead of one layer of yellow pine and one of oak as originally specified. This arrangement lessens the liability to rot.

3. In the sidewalks, four stringers were used instead of three, and here also the longitudinal spiking pieces were omitted and oak wearing surface was used instead of white pine.

4. The loads required in the original specifications were adhered to with the exception of the concentrated load for the roadway, for which a 15-ton Aveling & Porter road engine was used in preference to load of 12 tons on two pair wheels spaced 10 feet apart. An electric car such as used on the Pleasant Valley Railway, loaded, may weigh very nearly 13 tons, the wheel base being 7 feet.

The trusses were proportioned for a live load of 4000 pounds per foot of bridge.

5. Instead of the unit strains given in the original specifications the following were used:

Tension.

$$\text{Steel, } 12{,}500 \left(1 + \frac{minimum}{maximum}\right)$$

$$\text{Iron, } 10{,}000 \left(1 + \frac{minimum}{maximum}\right)$$

Laterals, 20,000.

Floorbeams, 12,500 net section.

Rolled steel I's, 16,000.

Strain from wind, live and dead loads not to exceed 25,000 pounds.

Compression.

$$S = \frac{P}{1 + \frac{l^2}{cr^2}}$$

in which $P =$ for steel $12{,}500 \left(1 + \frac{min.}{max.}\right)$

iron $10{,}000 \left(1 + \frac{min.}{max.}\right)$

Laterals, 12,500.

C = for square bearings, 36,000.
for one pin and one square bearing, 24,000.
for two pin bearings, 18,000.

The use of the above unit strains had the effect of reducing somewhat the amount of metal in the trusses. The metal thus saved was placed in the floor systems as above noted.

These formulæ are derived from the well-known "fatigue" formula of Launhardt, which is supposed to make an allowance for oft-repeated stress. While the true use of the "fatigue" formula is only to obtain the breaking load for metal often strained beyond its elastic limit and is therefore not properly applicable to any bridge member, it was used in the above form in the design of the trusses of the Ninth Street bridge, for the purpose of increasing the unit strain in such members that are rarely, if ever, subjected to the maximum load. Such members are the upper and lower chords of all highway bridges.

By the latter part of October, 1889, all matters were adjusted and contracts signed. The bridge as finally decided upon, consisted of three centre spans of 205 feet, and two end spans of 152 feet 6 inches, centre to centre of pins, and a viaduct approach on the Allegheny end 80 feet long. In accordance with the provisions of the agreement, the contractor was required to take down the old structure, do all the substructure and superstructure work ready and complete for travel. On account of the desire of the company to put their electric railway into operation at once, the contract required that the roof and lateral system of the old bridge be taken down immediately in order to give sufficient clearance for the electric cars. The work was to be commenced at once and completed by the 1st day of March, 1890. See Plate VII.

The contractor during the erection of the bridge was to make sufficient and suitable provision for the accommodation of all traffic, which was very heavy and going all the time, so that it would not be obstructed.

The usual restrictions in regard to keeping navigation open were imposed upon the contractor and he was also made responsible for all injuries to persons and so forth.

The work to be done, as required by the specifications was as follows:

SUBSTRUCTURE.

1. A new abutment to be built on the Pittsburg side, situated about 40 feet north of the old abutment.

2. The tops of all the piers to be rebuilt. The masonry for a distance of about 9 feet from each end to be removed to a depth of about four (4) feet and new coping stone placed thereon. On the area thus described it was the intention to place two layers of steel I-beams in order to distribute the pressure of the end posts as much as possible. It was afterwards decided to do away with the beams and use stone entirely. The skewbacks of the old arches to be removed and the tops built up to give the piers a uniform section.

3. A new pier on the Allegheny end to be constructed.

4. The old abutment on the Allegheny side to be removed, twelve pedestals for the new viaduct to be built and the top of the old retaining wall on the north side of the Pittsburg and Western Railroad to be rebuilt to suit the new stringers.

SUPERSTRUCTURE.

The bridge to consist of three spans of 205 feet and two spans of 152 feet 6 inches centre to centre of end pins.

The style of truss is the plain Pratt truss with sub-divided panels and straight top chord. The trusses are 35 feet 8 inches centre to centre, and the upper and lower chords are 34 feet and 2 inches centre to centre.

There are 12 panels of 17 feet and 1 inch in the spans, 205 feet long, and 10 panels of 15 feet 3 inches in the spans, 152 feet 6 inches long. The floorbeams are riveted to the posts and suspender, and at each floorbeam latticed iron brackets extend out to support the footwalk. The trusses, with the exception of the counters are of steel, and the entire floor and lateral sytem are of iron.

The viaduct on the Allegheny end to consist of five spans of plate girder work of 16 feet each. The intermediate supports to be iron columns set on masonry pedestals. The roadway and sidewalks are supported by ¼-inch buckle-plates springing from the plate girders. On top of the buckle-plates should be placed a layer of concrete and then Belgian block paving stones.

All material used throughout the structure was thoroughly tested and inspected. The steel, except rivet steel was required to have an ultimate tensile strength of not less than 60,000 pounds, nor more than 68,000 pounds per square inch; an elastic limit of not less than 33,000 pounds per square inch; an elongation in 8 inches of not less than 22 per cent., and show a reduction in area at points of fracture of not less than 45 per cent.

Rivet steel was required to have an ultimate strength of 56,000 to 60,000 pounds, an elastic limit of not less than 33,000 pounds per square inch, and an elongation in 8 inches of 25 per cent., and a reduction of area at point of fracture of not less than 50 per cent.

Wrought iron was required to have an elastic limit of not less than 26,000 pounds per square inch, an ultimate strength of not less than 50,000 pounds, and elongation of 18 per cent. in a distance of 8 inches.

In accordance with the terms of the contract, the contractor proceeded at once to prepare the old bridge for the passage of electric cars. The roof and side covering were removed and then the top lateral system taken out. To give the bridge lateral stiffness, temporary wooden towers were erected at the centre of each span, the verticals of which were thoroughly bolted to the arch timbers and floor and well braced above the top of the trusses. See Plate I. This work was soon done, and, until the completion of the shop work of the superstructure, was all that could be done except the masonry work. The new Pittsburg abutment was started about December 1, 1889. This abutment is located on the bank of the river, which is composed of filled material of all kinds. For this reason it was necessary to excavate about 26 feet to obtain a satisfactory sub-foundation. The bottom of the excavation is about 2 inches below extreme low water mark. Gravel was here found. In the pit thus excavated, concrete was deposited for a depth of 11 feet, on top of which the masonry work was started. The winter of 1889–1890 was extremely mild; at no time was it necessary to stop work on account of low temperature; but the prosecution of the undertaking was much hindered by the unstable condition of the river. The Ohio River and its tributaries started early in the year of 1890 to hamper bridge builders and kept up its erratic movements throughout the year. A brief description of the Allegheny River during this year is as follows:

December, 1889—Two rises above 13 feet; 11 days above 10 feet.

January—Three rises above 13 feet. For 18 days the river was above 10-foot mark.

February—Five rises above 13 feet; for 20 days above 10-foot mark.

March—Two rises above 13 feet, one of which reached 23-foot mark. For 16 days above 10-foot mark.

April—Two rises above 13 feet. For 12 days above 10-foot mark.

May—Four rises above 13 feet, one of which reached 20 feet. For 22 days above 10-foot mark.

June,
July, } Low water; average stage, 6 feet.
August,

September—One rise reaching 16-foot mark. Nine days above 10 feet.

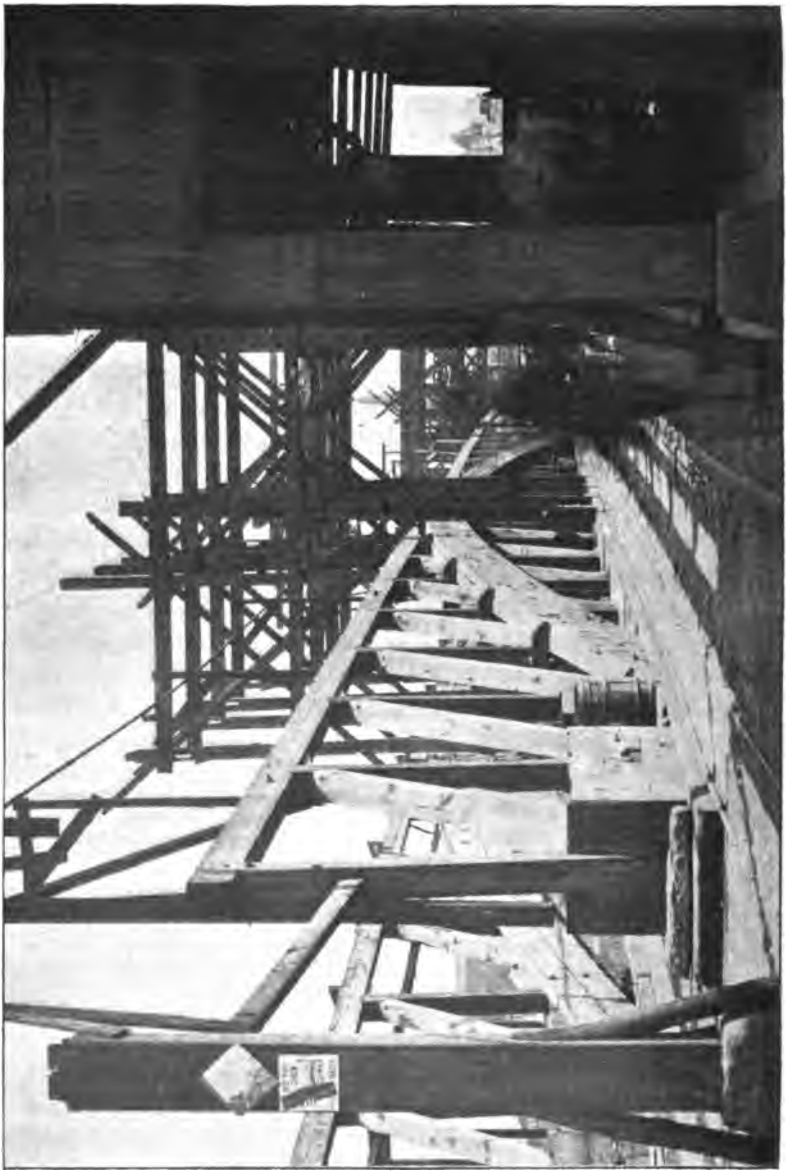
October—Two rises above 13 feet; 17-foot stage reached in each; 14 days above 10 feet.

November—Two 12-foot rises; 5 days above 10 feet.

December—One 11-foot rise; 1 day above 10 feet.

It will be observed from the above, that during the first five

PLATE I.



View from the Allegheny end, showing the old bridge and temporary lateral bracing after the covering was taken off.

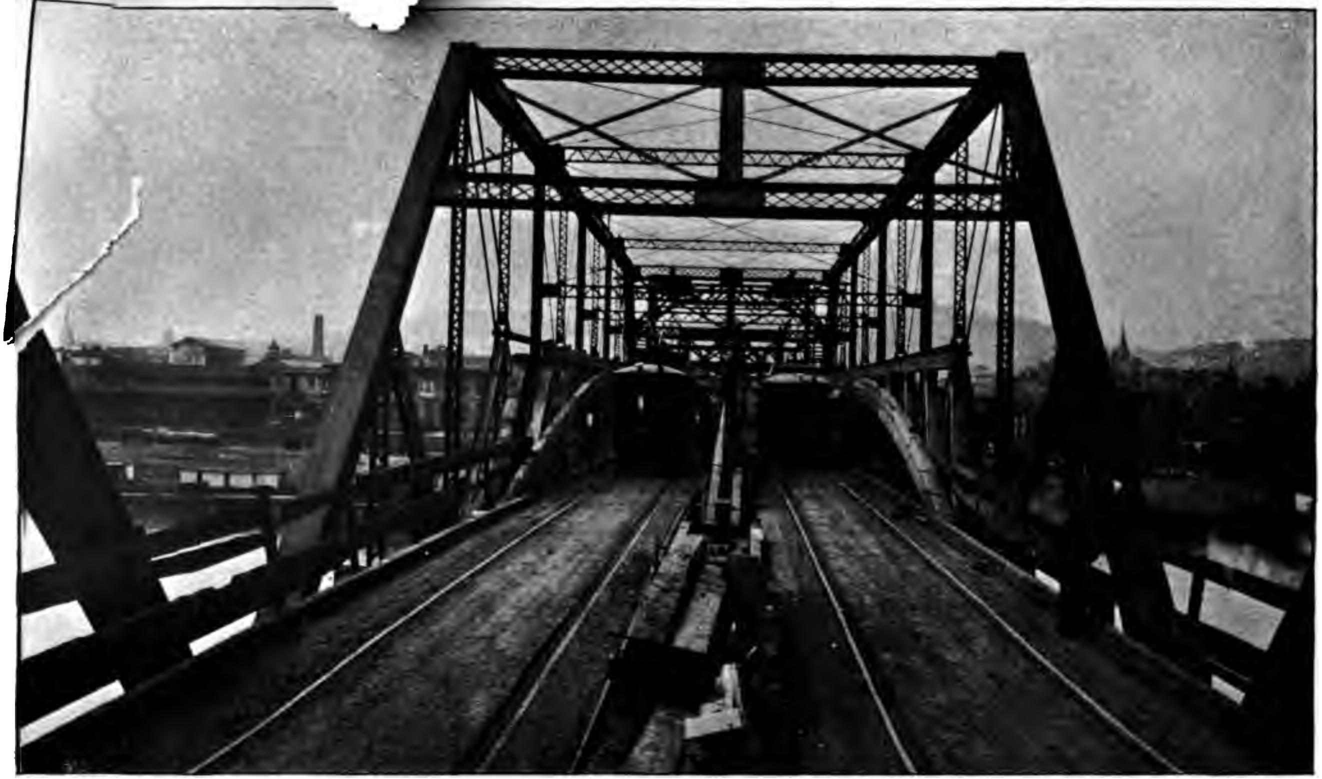
months of the year there were 16 rises of the river above a coal-boat stage, and during the 151 days of these months, for 80 days the Allegheny river maintained a stage of 10 feet. This was the part of the year in which our work had to be done, and it necessarily was very much retarded.

The new Pittsburg abutment, and especially the new pier on the Allegheny side, were very much held back until they had reached a height corresponding to a 14- or 15-foot stage of water. After they had reached this height they were readily completed, as there was sufficient room under the old structure.

In addition to the new abutment and pier a few pedestals could be constructed in Allegheny without interfering with the old bridge, but the repairing of the tops of the old piers and building of the pedestals located in the place occupied by the old Allegheny abutment could not be proceeded with, and had to be deferred until the taking down of the old bridge.

The removal of a number of the top stones of the various piers under the sidewalk revealed the inferior character of the masonry, and when this was established it was found necessary to change the method of procedure laid out for the erection of the new bridge. The new bridge was designed large enough to enclose entirely the old structure, the new shoes being placed against the old arches, and the top of the new coping being placed a foot or more below the skewback of the arches (see Plate II). On top of the new coping was to be placed two layers of steel I-beams as previously explained. It was proposed to make these 8 feet long and to extend them across the place occupied by the old arches. To prepare the piers and to insert the steel beams it was the intention to support one span at a time on false work, remove the arch and truss timbers and then rebuild one-half of the top of pier thus relieved. The thrust of the arch of the adjoining span was to be taken care of by the masonry left intact. The discovery of the character of the old masonry made clear the hazardous nature of such procedure. It was evident that the pressure of the arches on one side of the old piers would readily shear off their tops, and that some other method to sustain the pressure of the new spans on the ends of the piers had to be used.

PLATE II.

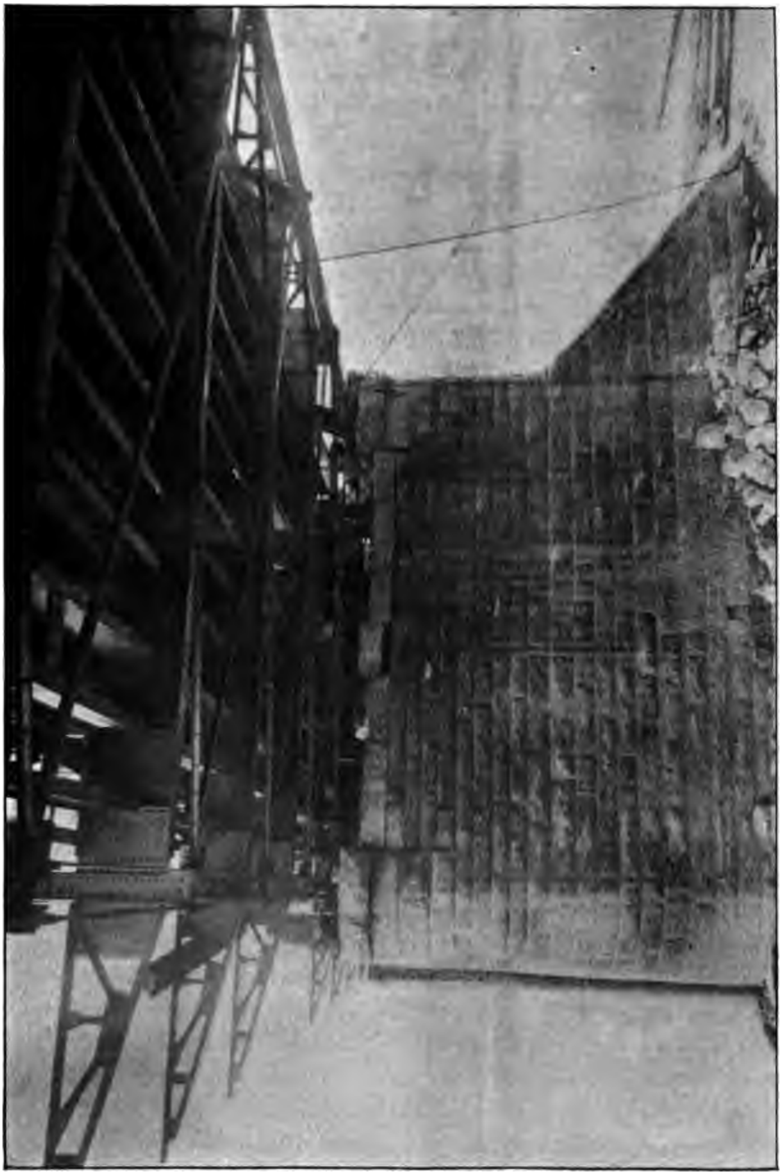


End view of an intermediate new span, prior to removal of old [illegible]

In order to maintain traffic, it would be impossible to take down any span of the old bridge until the thrust of adjoining span was taken care of, so that no unsupported horizontal pressure would be exerted against any pier. This, of course, made it necessary that the new bridge be erected without taking down the old arches until the false work of the adjoining span was in place and the weight of the roadway taken off the arch. After long deliberation it was decided to grout the piers, to cut down their ends as far as deemed safe, and rebuild to the proper heights with as large blocks of stone as could be handled. The grouting of the piers could be started at once, but the cutting down of the piers could only be done as the work of erection progressed. Three 3-inch holes were drilled into each pier, one at each end and one at the middle. The holes were about 25 feet deep, thus making the bottom of the drill-holes about 15 feet above low water. Water was run for five or six days through each pier, the object of this being to wash out as much old mortar as possible. The water was obtained by running a 2-inch pipe along the bridge from the Allegheny end. After this was done the joints and cracks in the piers were caulked, aud then grout run in the 3-inch holes. From 75 to 130 barrels of Domestic Portland Cement were used in each pier.

The work of cutting down the ends of the piers was done in as short a time as possible before the erectors of the superstructure started on any span. Tie-rods were used between the two outer arch timbers to prevent their spreading when the ends of the piers were cut down. These were cut down about 4 feet below the bottom of the old skewbacks and rebuilt with large blocks of oölitic limestone from Bedford, Indiana, set in Portland cement. Two stones were used in each course. Some of these contained 75 cubic feet and weighed 6 tons. They were placed in position by means of derrick-boats. As it was necessary to pass from one side of the bridge to the other, the mast of the derrick-boat was made very short, and the consequence was considerable difficulty in doing the work. The derrick broke on several occasions, and in one instance a large block of stone was dropped into the river after it had been hoisted nearly to the top of the pier. After having placed some of these stones, it was found that the old

PLATE III.



View under bridge, showing new floor system, and the new pier tops.

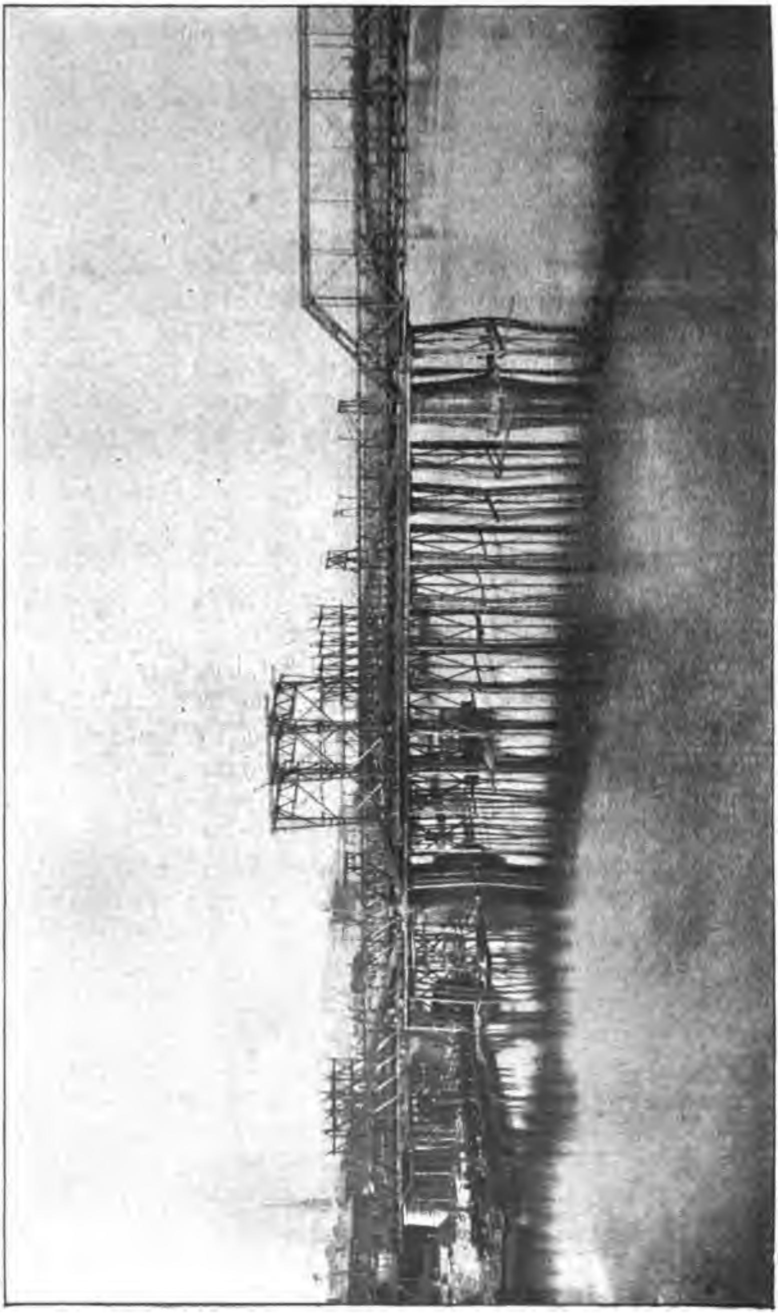
bridge could be used to good advantage for the purpose of attaching snatch-blocks and so on. A tremendous side-strain was thrown on the old arches, and some idea of how the old structure withstood wind-strains was thus acquired. Towards the latter part of our work we had obtained a great deal of confidence in the old bridge, and frequently used it in a way that would make many modern structures tremble.

The work of removing the old skewbacks and building up the pier to a uniform section between the new trusses was not started until the new trusses were erected and the old timbers entirely removed. When this had been done, a small derrick was erected in the centre of the roadway and the stones were lowered to the tops of the piers through holes in the old floor between the two tracks. There was about 4 feet clearance under the old floor, which afforded sufficient room for the masons to pry the stones to their places. See Plate III.

The work of taking down the old abutment in Allegheny, building the pedestals in the place occupied by the abutment and repairing the top of the north retaining wall of the Pittsburg and Western tunnel was easy of accomplishment, but, of course, could not be done until the old span of the Allegheny end had been supported by false work and the roadway of the approach from the north retaining wall to the face of the old abutment had been secured by timber beams placed longitudinally to the axis of the bridge. These timbers were held in place by blocking from the old abutment as it was being taken down. When it was entirely removed the beams were supported by timber bents. The masonry work on the Allegheny end could not be started until the work of erection of the new superstructure had reached that end, and was otherwise delayed on account of great difficulty in obtaining cars for the transportation of material.

The construction of the superstructure material was carried on in the shops during December, January and thereafter, and delivery of material began rapidly in February and March. In April, 1890, the erection started on the Pittsburg end. The material was all delivered on the Allegheny side and taken out in barges and hoisted into position.

PLATE IV.



Side view of the north end of bridge during construction.

The method of erection was as follows, one span being taken at a time:

1. The old sidewalks were cut off close to the trusses, the foot passengers being compelled to walk on the roadway until the span ahead was reached when they could pass to the sidewalk of that span.

2. The false work was erected. Plate VI.

3. The weight of the old floor was taken from the old trusses by blocking up from the false work. The old floor being at the time blocked up sufficiently high so as to permit the insertion of the new floor beams and stringers.

4. The ends of the piers were cut down and rebuilt as explained before. Plate III.

5. The erection of the truss and the placing of the top lateral system was done with a traveller as usual. Plate VI.

6. The intermediate floorbeams and stringers were then inserted underneath the old floor.

As the floorbeams next to the piers interfered with the old arches they could not be put in until these were removed.

7. The weight of the old floor was transferred from the false work to the new floor system.

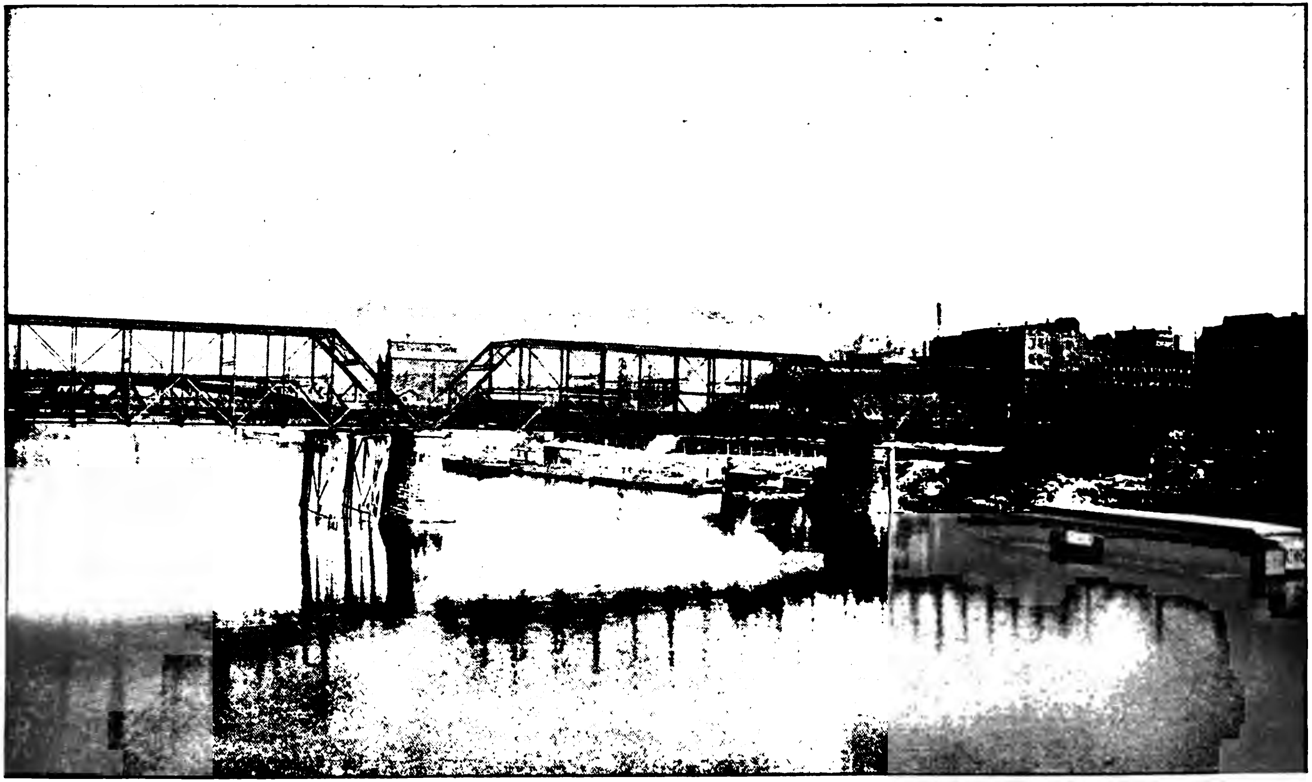
8. The false work, except the bent at the piers required to support the roadway at that point, was removed and placed under the next adjoining span. Plates IV and V.

9. When the false work under the adjoining span was erected and the weight of the roadway transferred to it, the old arch and truss timbers of the first span were taken down, the floorbeams and end stringers at the piers put in and the weight of the roadway transferred to them. The few bents of false work left standing were then removed.

This course of procedure was used in the erection of all the spans before laying the new roadway or sidewalk floors. A number of the sidewalk brackets, which were riveted to the outside of the posts and suspenders, were put in position, but generally all sidewalk work was left till almost everything else was done.

In the description of the old bridge mention was made of the fact that the vertical tension members extended about 18 inches below the bottom chord and that they occurred every 11 feet over

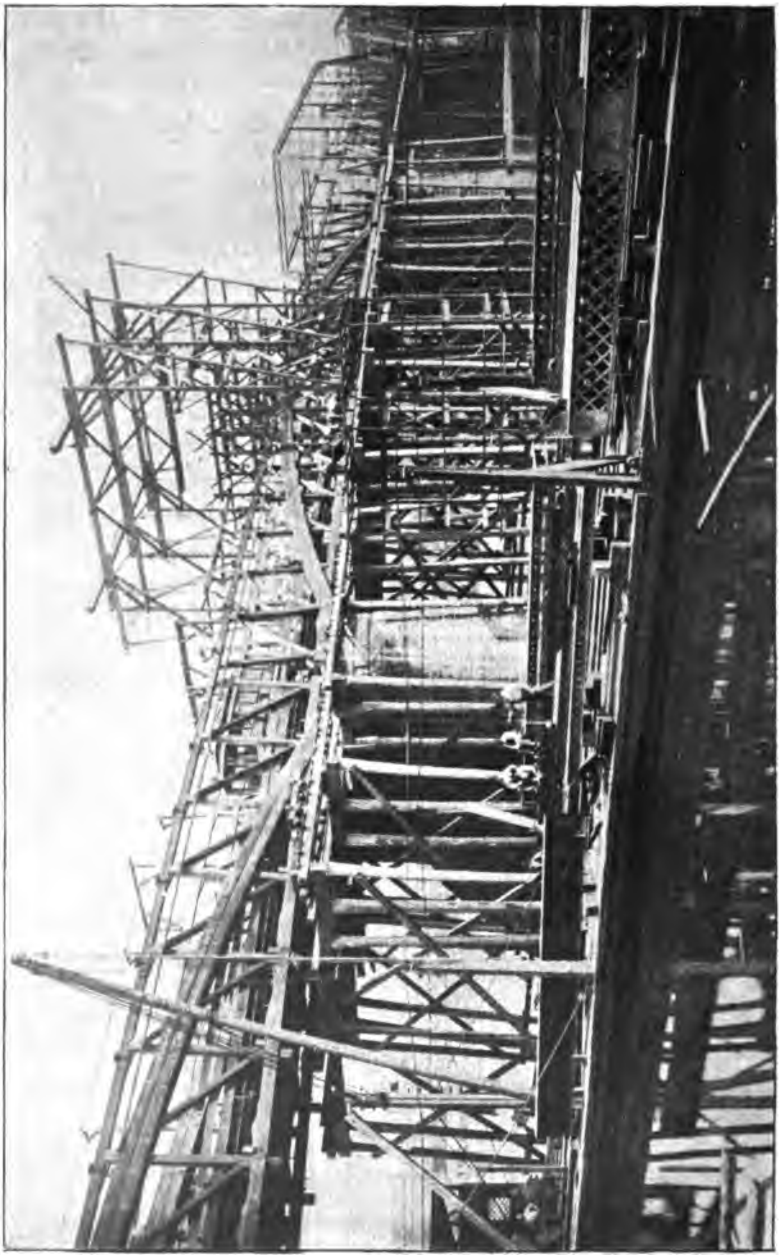
PLATE V.



Side view of the south end of bridge before removal of old floor.

the length of the bridge. In establishing the grade of the new bridge it was naturally assumed that the bottom of these members were the lowest part of the old bridge, and to retain the same clearance over the river, the bottom of the eye-bar heads of the new structure should be placed at the same level as the bottom of the tension members. This was done without referring the matter to the United States engineer officer in charge, as no difficulty was thought of. Two spans had been erected on the grade line so established when we were very much surprised to receive a notice from Colonel Merrill to stop erecting the bridge at the height adopted, and that it would be necessary to lift our bridge 18 inches to give it the same clearance as the old. In the correspondence that followed we were informed that it was customary for river men to so guide their craft as to take advantage of the space between the members projecting below the old chord, and that in certain stages the 18 inches in clearance so gained was of inestimable value. Such precision in steering was almost an incredible revelation to the writer. The now well-known congressional act of September, 1890, which gives the Secretary of War, through the engineer officers, complete control of all matters pertaining to our navigable rivers, with power to enforce same, had not yet been passed at that time, and the bridge company could probably have successfully held out against the order of Colonel Merrill, but, as the cost of lifting the structure was not very large, it was deemed best to obey the command without resistance. The height required to be lifted at the channel span was changed to 1 foot. In furtherance of this order granite blocks, 12 inches high, were placed under the shoes of the spans resting on the channel, piers and blocks 7 inches high under the shoes on the adjoining piers. The extreme ends of the bridge were not lifted. The raising was done by means of four 30-ton hydraulic jacks at each end post, one end of a span being lifted at a time. The jacks were applied to a built beam, 8 feet long and 24 inches deep, which spanned the shoe directly over the end pin. The beams were connected with the ends of the pins by 2¼-inch square iron hangers. The lifting strain was thus applied to the end pins without any danger to the structure.

PLATE VI.



View of north end of bridge from Allegheny side, showing old arches and trusses, also traveller and false work used for erection of new trusses.

Although only two spans were erected when the order for lifting came it was best, so as not to interfere with the established plans of erection, to erect the other spans at the height originally intended and afterwards lift up to final position. The jacking up was done very expeditiously and without at all interfering with traffic.

After this operation was accomplished the large floorbeams, resting on the piers between spans, were put into position and the placing of the sidewalk stringers and roadway floor proceeded with. By shifting the blocking between the old floor and the new floor system the transverse layer of 3-inch timbers, comprising the bottom layer of the new floor was readily inserted from the side and fastened from underneath to the top flanges of the stringers by means of wood screws. When the bottom layer was put into position the old floor, on which all traffic passed, was lowered to it by taking out the blocks. Then, by shifting the old floor to one side of the new structure, sufficient space was obtained to put down two tracks and to divert the traffic to them, after which the old floor was entirely removed and the remaining two tracks and the upper layer of the floor put down.

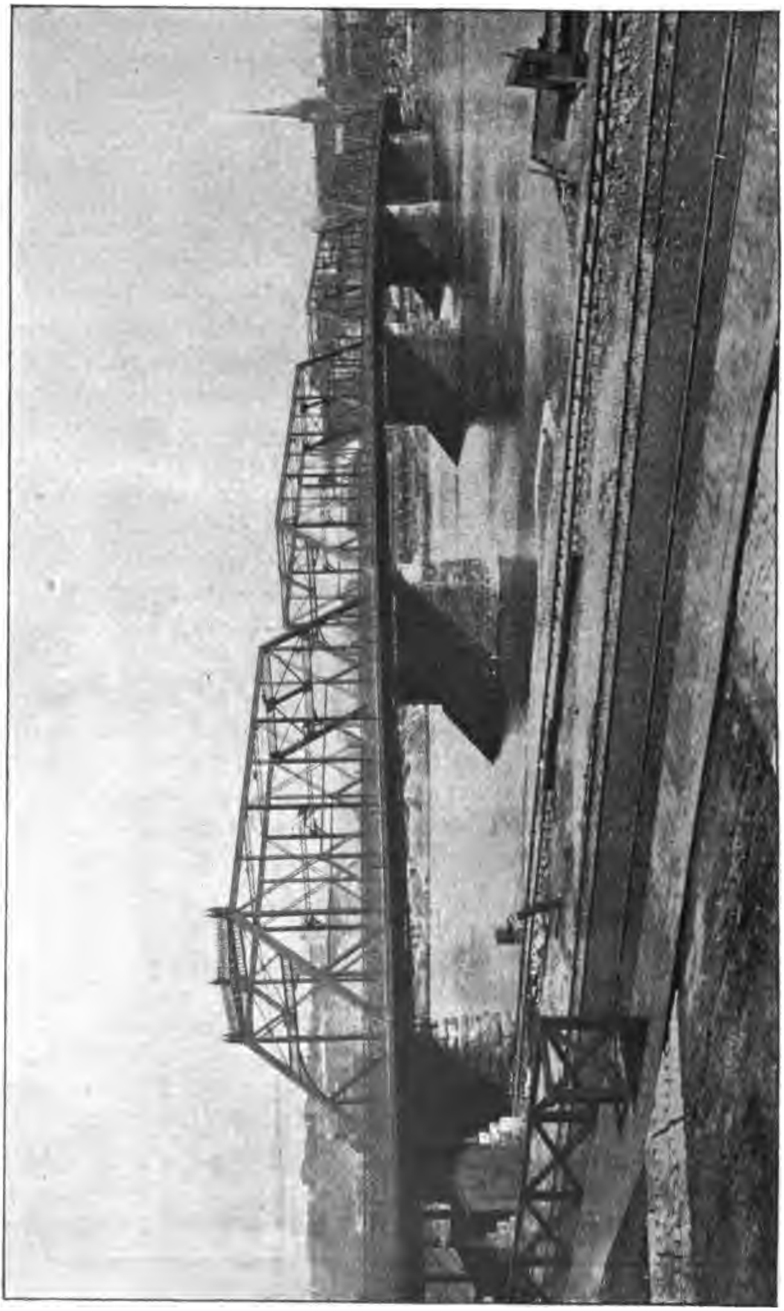
The work was entirely completed late in the fall of 1890 without injury to any person, and practically without cessation of traffic. For one-half day only was traffic stopped, due to the washing out of false work in May, 1890, when it was thought better, on account of the uncertain condition of the old structure, to take no chances until additional lateral bracing could be put in.

In conclusion the writer desires to say that the taking down of a bridge and erecting a new one, at the same time maintaining an incessant traffic, is an undertaking attended with much anxiety, especially when it is necessary to pin your faith to a timber structure of fifty years' standing and of which nothing is known as to its condition or strength.

DESCRIPTION OF PLATES.

Plate 1.—View from Allegheny end, showing old bridge after covering was taken off and the temporary lateral bracing.

PLATE VII.



View from Allegheny side of completed structure.

Plate 2.—End view of an intermediate new span, prior to removal of old trusses and floor.

Plate 3.—View under bridge showing new floor system and method of building up of pier between trusses.

Plate 4.—Side view of north end of bridge during construction.

Plate 5.—Side view of south end of bridge before removal of old floor.

Plate 6.—View of north end of bridge from Allegheny side, showing old arches and trusses, also traveller used for erection of new trusses.

Plate 7.—View of completed structure.

DISCUSSION.

PRESIDENT HUNT : We are all indebted for a very interesting history of a local structure. It has an historical as well as an engineering interest, especially as this is one of the first, if not indeed the first, of this class of changes in bridge work due to the new and heavier loading of electric and cable cars, particularly in the way of rapid transportation in cities. There have been since that time quite a number of the old Burr truss bridges changed, and undoubtedly there will be in the near future many more of them that will have to be changed. One of the great troubles with the old Burr bridge is that there is not enough head room for the electric cars, the top lateral system being too low down to allow for the increased heights required for the new electric cars.

As a matter of fact, however, the old engineers in their calculations of strains have been so successful in their work (or rather so extravagant in their excess of sections) that loads that would try our new and modern lighter iron and steel structures, to my knowledge, have been placed on several of these fifty and sixty year old structures with impunity.

There have been quite a number of these bridges in our own vicinity changed, as well as in other parts of the country, with still the upper braces standing. They have simply raised the posts and lateral bracing, undoubtedly to the detriment of the wind bracing of the bridge.

MR. THOMAS P. ROBERTS: The other Burr bridges to which you refer are probably not as heavy structures as this one. I took a great deal of interest in Mr. Kaufman's paper myself because I had some little personal connection with that bridge in past years in some work of repairs. I do not know whether or not Mr. Kaufman observed that several of the arch timbers had been pieced. I put in about ten feet next to the wall-plates, cutting the decayed timbers off. We had a great deal of difficulty in the cutting. The thrust was so great it would catch the saw and it would be absolutely impossible to move it, requiring that the wood be carefully blocked off.

On another occasion the first pier from the Allegheny side on the upper end I found caved in. As Mr. Kaufman mentioned, the old mortar had become disintegrated, the water rising at different times having acted on the lime and washed it out, leaving the joints rather open. I ran in about fifty barrels of cement in the form of grout in that pier.

In regard also to the wood, in looking at the arch timbers after the bridge had been torn down, one day I got permission to have specimens selected for a gentleman who was with me. He is an expert violin maker, though an amateur. He makes beautiful work and he selected a block from that bridge from which three violins have been made. The top portions of violins are made from pine (probably not all of our members may be aware of that). This old block proved to be better wood than he could find in New York. The instruments he made, were worth fully $150 each. The theory upon which these experiments was tried was that the almost constant vibration caused by the travel in these timbers was an equivalent to many years' playing on a finished instrument, and so far as results proved in this instance the theory had good grounds for support.

MR. W. G. WILKINS: I understand from Mr. Kaufman that the grouting of these old piers was done by pouring in on the old masonry. In what condition did he find that grouting after the joints were thoroughly well filled with cement?

MR. KAUFMAN: The top joints were found well filled with cement. We could not tell about the grouting below. It is natural to assume that the joints at the lower part of the piers were not filled so well as the top ones, on account of the cement and water separating, and the latter running ahead of the cement. Where we found the grouting afterwards we found everything well filled and very hard to take out.

MR. THOMAS H. JOHNSON: I think Mr. Kaufman misjudges the behavior of grouting under such circumstances. My experience has always been that the cement and sand find bottom first; and in this instance where he was working under hydrostatic pressure, pouring in through these holes, I should expect the bottom of the wall to be filled first.

MR. KAUFMAN: If that is correct, we made a better job than I thought.

MR. WILKINS: In what proportion was that grout made?

MR. KAUFMAN: It was pure cement, no sand at all.

MR. JOHNSON: There was one thing about the old bridge I would like to ask about to see whether any attention was paid to it. As I remember it, the sustaining rods which connected the old wooden arches to the lower chord of the truss were attached to the chord by a very peculiar arrangement, in which an iron bar was made to do duty more as a beam than as a hanger. It always had a suspicious look to me.

MR. KAUFMAN: I believe Mr. Roberts can give you more information about that than I can, as I think he inserted several of these hangers.

MR. JOHNSON: As I remember that hanger it was something like that (illustrating on the board).

MR KAUFMAN: They were attached to the bottom chord of the truss.

MR. ROBERTS: The object was to take the place of the counter-brace. The original bridge had no counter-braces, and this acted as a stiffener between the bottom chord and top of the arch. There was one span in particular, the upper side of the Allegheny span, with the arch, that moved very perceptibly, and with a single horse it was much worse than with a pair. We found it improved the bridge very considerably so far as taking out the vibration was concerned.

Another bridge of that kind is at Brownsville, the span being about 212 feet. I was called upon, being in that neighborhood building a lock, to look at it one day, and found that a corner of one of the abutments was ready to fall down. It had settled to such an extent that the arch did not touch it at all, hence such a strain had been imposed on the lower chord that it was pulled apart and the whole thing was in a very threatening condition and ready at any moment to fall down. I built an entire new abutment, put in an extra line of arches, spliced the lower chord and now it is all right. The great advantage of the Burr bridges is overlooked, that they are adapted to wooded countries where iron is scarce. Everything seems to be entirely of wood, even the pins. As to durability, when well roofed they have certainly proved their value in that respect. The bridge at Brownsville is now over sixty years old and is apparently as good as when built. The old Ninth bridge was probably good for ten years more with ordinary care.

MR. WILKINS: In regard to the preservation of timber, I would state in the construction of the number three pier on the new Sixth Street bridge when we made the excavation for the foundation we found the abutment exactly on the spot of the original bridge built in 1819. If we had had our foundation two feet either one way or the other from its site we could not have built our coffer dam of a size to get around that foundation. We found two courses of twelve inch timbers with the sap sides riveted together with oak pins. We took those out and quite a lot of the blocking used to raise the old floor is made of that old timber. When first taken out it had the appearance of new timber, not a sign of dry rot in it, but after thoroughly drying it out about one inch of the sap side showed marked signs of dry rot.

MR. HUNT: That was when the timber had been continually under water. With such timber have you not found that as soon as exposed to the air that it very soon rots? I have had one or two instances of that kind lately where the timber seemed to be as good as new timber when first dried, but it had hardly been out two months' time after it had been exposed to the air before it began to rot and very soon rotted almost the entire strength away.

Mr. Davison: If I understand the manner in which those hangers were put in, they were put in vertically to sustain the lower chord of the arch. This lower chord would be suspended from this arch without any counter-bracing whatever, if I understand these sketches. I do not remember noticing that about the bridge.

Mr. Johnson: The arch timbers of that bridge were bolted through to the posts.

Mr. G. S. Davison: The bolts would cause a reaction against the posts—was that the idea?

Mr. Kaufman: I think the explanation of that matter is this: The trusses were embraced between the timbers composing each arch. By the firmness with which they were held the two were made to act somewhat in concert for a time. The joints became loose and the arch would work by itself and the truss by itself to a small amount. The truss was not nearly strong enough to take the load. Its particular office was to act as a stiffening truss to the arch. When it became partially free from the arch timbers the least load, and especially a light load, would be carried by the truss and thus would cause a great deal of vibration. All heavy loads would be thrown to the arches by the deflection of the trusses. As soon as the truss was hung to the arch by means of the hangers the load on the arches was increased, and this held them down and resisted any vibration by increasing the dead weight. In that way the vibration from any loading would be very much less.

The same action can be seen in almost all the new bridges of to-day. Where there is a light load the vibration is usually greater than when the load is heavy and when a bridge is loaded to about its full capacity there is scarcely any vibration.

Mr. Hunt: I would like to ask you in regard to concentrated loads; you say that 13 tons is the present actual live load, and 15 tons is the maximum calculated load. Is not that pretty close?

Mr. Kaufman: The concentrated load of 13 tons is liable to occur, but rarely.

Mr. Hunt; Your uniform load on the roadway is 80 pounds or 100?

Mr. Kaufman: The uniform load is 100 pounds per square foot of roadway, 80 pounds on the sidewalk.

Mr. Hunt: I was especially interested in that point, as it is a live question to-day just what the uniform and concentrated loads should be for the new electric railways that are building over the country.

Mr. Davison: I would like to ask what assumptions were made in regard to the moving load?

Mr. Kaufman: 80 pounds per square foot, or 4000 pounds per lineal foot.

Mr. Davison: I understand that was the first specification.

Mr. Kaufman: It remained the same in the revised specifications.

Mr. Hutchinson: I would like to inquire whether this load is for one track or two? Is it on each track? Also another point: Mr. Kaufman stated that they raised the shoes of the channel span 7 inches and 12 inches by stone bolsters. I would like to inquire how large these stones were in the other dimensions.

Mr Kaufman: All tracks were figured for the concentrated load of 15 tons. The stone raising blocks were four feet square.

Mr. Hutchinson: Were there signs of any weakness on account of the thinness of the stones as compared to the other dimensions?

Mr. Kaufman: No, sir.

Mr. Roberts: Any indication of cracking?

Mr. Kaufman; No, sir.

Mr. J. A. Brashear: As this discussion and the paper itself have proved so interesting, in view of Mr. Robert's mention of the old Brownsville bridge, it seems to me that it would be interesting to this Society if Mr. Roberts or some other of our bridge engineers, when in that neighborhood, would obtain the history of the old tubular bridge across the creek there. I believe it was the first tubular bridge in the United States. I do not know how far back the date is, but I believe the old structure was built in Brownsville in 1822. It is certainly a very interesting structure, and I do not think its history has ever been given in any

Society in Western Pennsylvania. I should think no one can do it better than Mr. Roberts.

Mr. Roberts: Mr. John M. Goodwin, the first trip he made up the Monongahela River with me,.was much struck with this bridge and he proposed to get in correspondence with some person about it. I made an attempt to gain some information regarding its history, but nothing could be found in Brownsville. It is the sleepiest place in the United States. It consists of five lines of elliptical cast-iron arches, very thin, about five feet high and less than two feet through. It is beautiful in shape and of perfect castings. I think each segment is probably six or eight feet long, united making a span of about ninety feet. The roadway is paved with stone flag pavements. To-day the corner masonry lines are just as true as the day the bridge was finished, showing there has been no settlement in the foundation work. It was built in 1822 by the United States engineers in charge of the great national road, a good history of which should be forthcoming.

Of course the records and plans would not have been left in Brownsville. The proper place to look would be in Washington. No person in Brownsville can give any information regarding that bridge, and I believe it stands to-day as the only bridge of that kind in the United States, although there are some in Germany.

Mr. Johnson : Referring to that bridge I believe it is said to have been the first iron bridge that was built in the United States, and reference having been made to similar bridges in Germany, I may state that I have seen this bridge illustrated in an old volume of the *Allgemeinen Bauzeitung.*

Mr. Hunt: It is the same shape as the Menai Straits tubular bridge in Great Britain.

Mr. Brashear : I understand that Colonel Searight is to write a history of the old National turnpike. As the turnpike crosses that bridge he may be able to give us some light on it.

Mr. Roberts: There is one point you brought up yourself Mr. President, about the bridge that is interesting, and that is in reference to the old timbers taken out from under the water Does it improve the timber? That is really a question of some importance in a great many ways. I think myself, and I agree with you, that for external use it is not as good as freshly-sawn timber. I think some molecular change in the arrangement of the fibre occurs, although it does not injure it for musical tone, as that is demonstrated from the fact of the violins I referred to. I here refer to those parts preserved under roof above the water.

Under a microscope timbers from our dams on the Monongahela river which had been under water forty-three years seem impregnated with silica. It is hard on tools, as it dulls them easily. There was something in the fibres that looked like a mineral deposit. That would not be the case with timber preserved like this old bridge timber. I should think all old well-preserved timbers would be valuable for inside work where fine fittings were required, and where the question of shrinkage was important, and the timber in that bridge, as Mr. Kaufman states, was carefully selected white pine from the Allegheny. The best pine we ever had in the United States came from the Allegheny Valley, I think, better in texture than the Wisconsin and Minnesota pine. But none or very little of that is to be had now.

MR. KAUFMAN: The old timber was used by the Company in building a car house in Pleasant Valley.

MR. HUNT: Is it standing the weather?

MR. KAUFMAN: It is covered and not exposed to the weather.

MR. W. L. SCAIFE: Have any experiments been made as to the tensile and transverse strength of this timber which was taken out? It would be interesting to know if vibration had affected the timber and to what extent.

MR. KAUFMAN: At the time of taking down the bridge the matter was thought of, but it was concluded not to take any time to it because we were of the opinion that but little satisfaction could be obtained in comparing such tests with the imperfect timber tests at present on record.

MR. JOHNSON: On that subject, a few years ago we had a wooden bridge upon our lines west of Columbus that had been standing for I think at that time, about twenty-five years. Externally the bridge appeared to be perfectly sound, but on account of its age the question was raised whether it was safe to continue it in service for railway traffic. Mr. Becker had some of the braces removed and new ones substituted, and had that timber tested, with the result we got tests so much higher than any tests on new timber which we could find, that we considered the bridge perfectly safe and it is still in service.

Mr. Roberts: There is one interesting reminiscence of the old Ninth Street bridge worthy of note which Mr. Kaufman has not touched upon; he omitted mention of it perhaps because it was not of sufficient engineering interest. But in these days of "talk" about aerial navigation, perhaps the inventors can obtain a useful hint from a relation of the facts. Of course, I allude to the celebrated "Goose fly," noted in the annals of Pittsburg. Away back about 1847 there was as much talk about flying machines as there is in these brilliant days of engineering societies, and about that time a stranger from the east arrived in the smoky burg, and promised to entertain the public with an exhibition of a flying machine. After a while the papers became full of the promised experiment, and finally a day was mentioned in which the machine was to be "set in motion" from the promenade walk on the roof of the Hand Street bridge. The people were requested to assemble upon the river banks below the bridge, as it was stated the attempt would be made to fly from the Ninth Street (or Hand Street as it was then called) over the Sixth Street (or St. Clair Street bridge), to Temperanceville. Upon the afternoon in question the entire population of both cities—besides nearly every person from the adjoining counties assembled at the place designated. It is said that 40,000 were present. There was much tedious delay—as there always is at balloon ascensions and such like public exhibitions, but this was borne good-naturedly—and many were the remarks made by gentlemen "that they would not miss the expected sight for all the world." After a while the "committee" standing around the big box up on the middle of the bridge shouted "all ready—let her go!" and the next moment a big white goose was thrown into the air and sailed—squawking as it sailed—over the heads of the assembled multitude. The most remarkable feature of this early experiment in aerial navigation is, that even within a week afterwards, not a living soul could be found who had witnessed it—all had apparently died—save the newspaper reporters, and no epidemic was ever known to hurt one of them.

Adjourned. R. N. CLARK,
Secretary.

MINUTES OF MEETING OF CHEMICAL SECTION OF ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA, AT SOCIETY'S ROOMS, FIFTH STREET, PITTSBURG.

SEPTEMBER 27TH, 1892.

Vice Chairman, Charles B. Dudley, occupied the chair. Twenty-three members were present.

The resignation of the Chairmanship by Prof. John W. Langley was read by the Secretary. This action on Prof. Langley's part was made necessary by his removal to Cleveland to accept a professorship in the Case School of Applied Science. His resignation was formally accepted, and the following resolution unanimously adopted:

Resolved, That the Chemical Section of the Engineers' Society of Western Pennsylvania deeply regret the withdrawal of Prof. John W. Langley from their Chairmanship, and that they hereby express their obligation to him for his able service as presiding officer.

The election of permanent chairman was then suggested, and Prof. F. C. Phillips and Mr. George Faunce were nominated. After some discussion, these nominations were withdrawn and the election deferred till the October meeting in order that the members might be more generally informed.

The election of members of the two Standing Committees was also deferred.

Dr. Charles B. Dudley, Chemist to the Pennsylvania Railroad Company, then gave an interesting address on the subject: "Causes of Discrepancy in Chemical Analysis."

The causes, he thought, were as follows:

1. Non-uniformity of samples. To remedy this, or to discover the fact, chemists should exchange samples.

2. Impurity of chemicals. In case of difference of results, chemists should exchange and examine their *chemicals* to locate trouble.
3. Poor manipulation.
4. Different methods.

His address was full of illustrations and was of the greatest interest. It was discussed by Messrs. Ford, Eastwick, Carnahan and Camp.

ADDRESS TO THE MEMBERS OF THE CHEMICAL SECTION OF THE ENGINEERS' SOCIETY AT PITTSBURG, SEPTEMBER 27, 1892.

BY C. B. DUDLEY, CHEMIST PENNSYLVANIA RAILROAD COMPANY, ALTOONA, PA.

GENTLEMEN: Our worthy Secretary asked me to prepare a paper for this meeting, but I have been so pressed with work that I have not had time to put anything in writing. If you will bear with me fifteen or twenty minutes, or may be ten minutes, as matters shall come up, I would like to talk about a thing that has caused me a great deal of thought lately, and in fact, has led to some positive action which may or may not meet with the approval of the profession a little later on.

My theme is: "Discrepancy in Chemical Work by Different Workers." What are the causes of discrepancy in results obtained by different chemists?

I have recently seen a series of, I think, sixteen determinations of sulphur in a piece of pig-iron, supposed to be the same iron; that differed from each other from 0.005 up to 0.02 per cent., or the extreme results (I am giving these figures from memory) were about as 1 to 4. Now, obviously, while the amount of sulphur is excessively small in this case, not being a matter of very great importance, yet, as bearing on the accuracy of chemical work, the result is something appalling. I have seen a series of phosphorus determinations recently, made by six or seven chemists, where the extreme results differed 0.03 to 0.04 per cent. in a total of about

0.10 per cent. I have a friend, who, for a number of years, was manager of a large furnace, who some four or five years ago, sent out borings from some pig-iron to eight or nine different chemists for phosphorus determination,—this is another case besides the one I just referred to—and when he got the results back, no one of the chemists knowing that any other was working on them, they differed almost as 1 to 2; and, in his nervous, energetic way, he said: "'I said, in my wrath, all chemists are liars!'" Perhaps it is not necessary to mention any more discrepancies; I might, however, give one more instance. In a recent analysis of bronze, we obtained, in our laboratory, a trifle over 9 per cent. of tin; another chemist, working on what was supposed to be exactly the same metal, being half of the same pig, got over 10 per cent., the discrepancy being about 1¼ per cent. Now, obviously, there is something wrong somewhere. Why is it that we get such discrepancies? Undoubtedly, most of you have reasons quite ready to explain some, at least, of the discrepancies. I have gone over the subject recently, and, as I philosophize, the causes for discrepancy in chemical analyses may be grouped under four heads. You shall say, after I am through, whether there are more; whether I have covered the ground, or have left something out, or whether I have got too much in. I will say, by the way, in order not to mislead you at all, that a portion of what I am about to say has been published recently in an article in the *Railroad and Engineering Journal* of New York; not with the same dilution, perhaps, that I shall give it to you, but the gist of the thought. I wouldn't like to get up here and give you something as original that has once been published.

The first cause of discrepancy in chemical work is that the two chemists did not work on the same sample; or, in other words, non-uniformity of samples. Upon this point, your own experience will doubtless give each of you an illustration. I do not have in mind now anything occurring out of our own personal experience in our laboratory work at Altoona, now nearly seventeen years of it, where there has been serious discrepancy due to difference in sample. The nearest we have come to it is this: At one time we were buying spiral springs on specifications that the carbon should not be below .90 per cent. The springs we examined were made out of a steel wire about a quarter of an inch in diameter, the coil being about an inch and a half across, and five or six inches long, what we call "A" springs, and used to hold the box lids tight to the oil boxes under the cars. We found in those springs so low carbon, in a number of cases, that we rejected them. The manufacturer got some one else to determine the carbon in the steel, and found the requisite amount; and so, of course, the question came up for an explanation as to the discrepancy. The discrepancy was very easily explained, and we ourselves, in our later work, found the same peculiarity in other steels; the borings required for analysis of these springs (they were unhandy, unwieldy things to bore, being small) were simply what we could get handily, mostly from the outside of the wire. The manufacturer took the same springs, had the outside turned off, and then took his sample from the centre of the wire, and this was the cause of the discrepancy, as we have proved by a number of test analyses. Apparently the outer layer of a steel rod that has been heated, as is commonly done with spiral springs, very frequently loses .10 per cent. of carbon in the fire. Or it may be, segregation during cooling explains the difficulty. At any rate, we have several times done this, viz., take a wire rod, $\frac{3}{4}$ of an inch in diameter, and have the boring done from the side, not the end, with a $\frac{5}{8}$-inch drill, may be $\frac{3}{16}$ of an inch deep, and then make a carbon determination from these borings, and then take $\frac{3}{16}$ of an inch more toward the centre and make a second carbon determination ; we never get the same carbon in the two samples. This is the nearest to an easy illustration that I can give you of discrepancy due to difference in sample. Of course, it is perfectly obvious that if the samples differ, there is a legitimate and good reason why the analyses should differ. This is especially true of shipments that are made up of large quantities, and are sampled by the single sample. We have many times had this brought to our attention by the men at the shops. Samples taken out of the same lot of material may not show up the same. They say it is the same material because it came in the same car. This is especially true in oil samples. It is not at all rare for a manufacturer who receives an order for fifty barrels of lard oil, for example, not to take that oil all out of the same tank, although he ships the fifty barrels in the same car. We have had a number of cases where the amount in tank did not suffice to fill the order, and, consequently, some was taken out of another tank, and sometimes inferior material was put in—two or three barrels, as the case might be—to fill the order. But I need not go further upon this point. You will all recognize that there is a clear and sharp reason for discrepancy in analyses if the samples are not the same; and in all our work, when we come to a serious difference between ourselves and any one else, we exchange samples. That is one of the first things we do,—exchange samples to see whether the difference lies there.

The second cause for discrepancy in chemical analysis is impurity in the chemicals. I doubt not many of you have run across this peculiarity. One of the most recent we ran across was this: In mixing up some wash water of sulphate of ammonium and free sulphuric acid for washing the yellow precipitate in phosphorus determinations, we ran across a very puzzling sort of trouble, namely, after washing a few minutes the filtrate became turbid, and the more we washed the more turbid it became, and we were inclined to think that the published statements of the insolubility of the yellow precipitate in sulphate of ammonium and sulphuric acid wash water were probably fallacious; but on looking into the matter we found that the commercial sulphate of ammonium we had used had a little phosphorus in it.

There are many discrepancies in analyses due to the impurity of the chemicals used, and it is not always an easy matter to say whether the chemicals are pure or not. I must say frankly to you that the longer I work, and the more experience I get, the more I am inclined not to be so sure as I used to be. Obviously, there are two or three methods of checking up whether the chemicals are pure or not. One of the most common is simply to test the chemicals; but I do not know how anybody would have found the impurity by that method, which was run across in the work of the International Committee on Standards in carbon determinations, namely, the presence in chloride of ammonium of a little organic matter, that only comes out when you dissolve steel in the double chloride of copper and ammonium. Another method is to make a dummy analysis, using a second beaker, to which you add only the reagents that you use in the one containing the substance to be analyzed, and then weigh up what you get from the two, and deduct from the genuine analysis what you find in the dummy. There is always a little uncertainty in that, namely, have the same reactions taken place in the beaker that has not got the substance in to be analyzed that have taken place in the beaker that has the substance? There is one thing more present in the beaker that has the substance in, and this additional thing may introduce changes in the final result. The dummy analysis is not always quite certain; in many cases it undoubtedly is. I would hardly feel willing to say that you will always catch up the impurities of the chemicals by the dummy analysis, nor would I think that you could always catch up the impurities of the chemicals by a test, as it is so difficult sometimes to test them.

This is a thing that all of us, undoubtedly, need to pay a great deal of attention to. I would like to call your attention to an instance. We had some so-called C. P. molybdic acid from five different sources recently; three of them had ammonia present; one of them was free from ammonia, but there was a little soda or some other soluble base with it; and one of them was pure molybdic acid, as far as we could get at it. Such peculiarities as these many times may possibly cause discrepancy. It is evident that in making up a solution of molybdate of ammonia, if you have something called molybdic acid, and put in the amount required of this material, and it is in reality molybdate of ammonia or of soda, you do not get the same strength of solution you would have if genuine molybdic acid had been used. Our experience indicates that this point cannot be ignored in phosphorus determinations. Your own experience will, of course, do two things for you; first, warn you (has already warned you undoubtedly) never to trust results of analysis unless you have checked up the chemicals, and, second, will convince you that there are many impurities in so-called C. P. chemicals.

A third cause of discrepancy in chemical analyses is what may generally be called "poor manipulation." I know of chemists who think this is the principal cause. The main cause of discrepancy in chemical analyses, some say, is poor manipulation, or lack of skill. My old teacher in chemistry, who has now gone out of the business, and lives on a farm in California, was characterized by this one very remarkable peculiarity, namely, he never believed anything as long as there was a shadow of doubt, and after all known uncertainty had been removed he was not quite sure; in other words, he was a man who was really a struggler for accuracy. He used to say to me in his very dry way. "No chemist can make an accurate analysis. There are chemists who can work near enough to accuracy so that their work is valuable. There are chemists who can not. And that is the difference between chemists." Of course, what I mean by an accurate analysis is a question of limits. Some analyses are accurate to a half per cent, some to a tenth of a per cent., and some, perhaps, to a hundredth of a per cent., but none, I think, are the exact truth.

The point I want to make, however, is not a dissertation on accuracy, but the difference between chemists due to manipulation. Undoubtedly, this is a very frequent source of error. I remember very well when I was a student, I was set to make a determination of iron. I dissolved the substance in sulphuric acid with the utmost care, excluding the air, and, at the end of the operation, after going through the necessary routine, I filled up my flask to one litre, shook, and supposed I had a solution that was in every sense what it should be for the subsequent operations, which were to draw out successive portions of 100 cubic centimetres, and titrate them with permanganate of potash. To my surprise, the first two that came out differed from each other from 10 to 15 per cent. What was the difficulty? Why, simply, I had not mixed the materials. I thought, in my inexperience, that if they went into the same flask, it was all right if I gave it a shake and a stir. Let me give you another illustration. Recently, we had occasion, in our laboratory, to make some tests for phosphorus, and I set two or three boys at the same thing. We knew pretty well from good careful manipulation and fairly well tested methods, what the steel contained, and the directions were to wash the yellow precipitate until the wash water tested with sulphide of ammonia showed no change of color. We were washing out molybdic acid and iron salts, and, as you know, both the molybdic acid and the iron would show change of color with sulphide of ammonium. In some five determinations, one of the boys got (it was low steel, containing almost exactly 0.4 per cent. of phosphorus) .048, .047, .051, .043, .045. You will note the discrepancy, and yet he had followed the directions, as he supposed, with absolute accuracy.

There are two points involved in this illustration, namely, to show how the manipulation may be at fault, and, also, however accurate you may be in giving directions as to how analyses shall be conducted, you may be thwarted by something you had not thought of. Well, on careful examination, we located the difficulty. The difficulty was simply this: the yellow precipitate had a little tendency to crawl, and this manipulator was a little bit afraid it would get over the top of the filter, and, possibly, get carried down through with the washings, and in his anxiety to avoid this he failed to wash all the molybdic acid out of the upper part of the filter. He supposed he was washing it out completely, but as the result showed by failing to wash to the top of the filter he removed so little with each successive addition of wash water that the amount was too small to react with sulphide of ammonium, so that although he accurately followed the directions, he still left enough molybdic acid (which you know, by the volumetric method, is what we are really measuring) in the filter to give a little high results.

I have no doubt that hundreds of illustrations will occur to you where manipulation comes in as an element of error. Directions are given, say, for example, to heat to a certain temperature. One man guesses at it; another puts in a thermometer. Again, the directions may be very indefinite. They may be, for example, "add a little of this or that," or "add a little, not too much," without giving any measurements whatever. Or the directions may be accurate—"add 5 c.c.," or "10 c.c."—and one man guesses at it, and another measures it. The manipulation certainly does affect the analysis. In filtering, one man slops a little, and another does not. All of these are points that come in, so that I think, without further question, you will accept this as one of the reasons why there are discrepancies in analyses, namely, failure on the part of one or the other of the chemists in accuracy of manipulation.

Now there is a fourth cause for discrepancy in analyses, if I understand it rightly, and that is the method. I think you will all agree, that two methods may not give exactly the same results. I have in mind a couple of cases which occurred quite recently with us. In a determination of tin in bronze, we never weigh up the metastannic acid as we separate it from the bronze by means of nitric acid. We have never succeeded yet in getting all the iron and all the copper away from the oxide of tin by means of nitric acid. Also if the bronze contains any phosphorus, it will, as you know (part of it, at least), go down with the tin. Even if you take an ordinary straight bronze that is simply a copper-tin alloy, and dissolve it in nitric acid, and weigh the tin just as you get it by separation in nitric acid solution we think your results will be a little high. So we dissolve our metastannic acid in sulphide of ammonium, filter and re-precipitate the tin sulphide; in this way we always get a little copper out, and sometimes a little iron. And although, as you know, we do not even with this care completely separate the copper from the tin owing to the solubility of copper sulphide in sulphide of ammonium, we think we get much nearer the truth than if we neglected this precaution. On the other hand, I have known cases where it is quite the custom to weigh up the metastannic acid just as it is separated from the other constituents of the alloy by means of nitric acid. Now here is difference of method. No one would think those two methods ought to give the same results.

I have another illustration: We have been accustomed for a little while, to determine the lead in alloys by precipitating lead on one of the poles of a battery as binoxide, according to Edward Smith's recent manual of electro-chemical analysis, a book which, by the way, each and every one of you ought to have and study carefully. I need not give you the detail of the method farther than to say that if you have copper and lead in nitric acid solution and apply the battery, the conditions being all right, the copper precipitates on one pole as metallic copper, and the lead on the other as binoxide. If we may trust our experience the results are excellent, if you have nothing else but copper and lead present. We took a known amount of lead and put in with it a known amount of copper, and get out almost the actual amount we put in, so that we think the method is capable of great accuracy. But in a recent analysis of a bronze containing lead we got 1 per cent. higher in lead than somebody else got by the sulphuric acid method, and the worst of it was that when we came to determine the lead by the sulphuric acid method, we confirmed the other chemists' results. It took a little time to find out where the difficulty was. I may say, for your information, that bismuth follows the lead, if it is in small amount; if it is in large amount, it will go with both the copper and the lead. We found a trace of iron in with the lead, and just the faintest trace of copper, a little tin, a trace of antimony, and strong suspicions, but not positive proof, of bismuth, apparently enough, in the aggregate, to account for the discrepancy. So we see that all methods of procedure are not equally good. All methods will not give the same results. "Method," therefore, is a fourth cause of the discrepancies in chemical analyses.

Now, if you will forgive me, I would like to spend just a moment on another thought that comes in perhaps under manipulation best, but almost seems worthy of a place by itself, namely, there is frequent discrepancy, if we may trust our experience in chemical analysis, due to the fact that one chemist knows what he is doing, and the other chemist simply follows directions and doesn't know what he is doing. I have seen a good many chemists make chemical analyses whose minds seemed to be anywhere else except upon the analysis; whose minds did not follow the changes that were taking place; who did not understand the rationale of the process. I have seen other chemists whose minds were always engaged upon the changes that were taking place while they were making the analysis, even during the filtration. One thinks, "What are we washing out?" The other thinks only, "the book says, wash," and so he goes ahead and washes. He doesn't think what he is washing out; and usually doesn't test to see whether he has washed it all or not, unless, perchance, the book especially says so; he is thinking of something else. We had an illustration of this lack of thinking what is going on in the progress of an analysis which caused us a little annoyance recently. This very puzzling thing happened to us: We made an analysis of a tire and found about .14 per cent. of silicon in it. The analysis was made by the Drown method, dissolving in sulphuric and nitric acid evaporating until the sulphuric acid fumes, and then diluting with hot water and filter. You are doubtless all familiar with the manipulation. As I said, we found .14 per cent. of silicon in this tire. About a month after it was sent out we received a letter from a chemist of another railroad, saying that he had seen the analysis that we made of the tire, and that his determination of the silicon in the same tire was nearly double ours, and asked us to kindly send him our method so that he could check himself up and see where he was wrong. He was very nice about it, you see. Before sending him the method, we thought we would go over our own work again, and upon doing so, confirmed his results. I would like to say here, in parenthesis, that the average of our work does not cause us this trouble. We make a great many hundreds of analyses, and these are only a few cases, but they happen to illustrate my remarks, and it is better that I should draw my illustrations from my own experience, if possible. Where was the difficulty? Our subsequent work, I say, confirmed the analysis of the other chemist; we got .28 per cent., same as he did. On looking the matter over we found the difficulty. A great deal of work is done in the Pennsylvania railroad laboratory, and much of our work is the examination of shipments of commercial products that are bought for use on the road. Now, other peoples' actions depend on the results of our work. If, for example, we buy a shipment of 100 barrels of oil, none of that oil can be used, unless some very great emergency has arisen, until the laboratory report is furnished to the parties who are to use it, so that we must not allow anything, unless it is very serious, to interfere with our doing that work.

It happens, therefore, that the work on shipments takes precedence, and the investigation of miscellaneous samples takes our leisure time when we are not working on shipments. This was the case with the tire. The operator who had the matter in charge, apparently without thinking exactly what he was doing, since he was doing a great deal of other work, and had many irons in the fire, dissolved the steel in the regular way, evaporated to the fuming point (just as he should have done), but, then, instead of diluting with hot water and filtering at once, he diluted and let it stand. It happened it stood forty-eight hours. Now, it is possible that none of us would have thought that this would cause any trouble, but subsequent experiment showed that this was the explanation of the discrepancy. Apparently, the silicon obtained by the sulphuric acid method is not dehydrated completely. At any rate we made positive determinations subsequently on the self-same tire, and if we allowed it to stand forty-eight hours after dilution we lost about half of it. If we allowed it to stand six days we only got .06 per cent. I said to our boys that I was glad the thing had happened, because it brought out a point we would not have thought of easily otherwise. Nevertheless, it seems to me that a chemist who thinks much, and who carries his work with him, and understands what is going on, would have thought that possibly there might be danger in the delay.

Now, we come to perhaps the most important point in our whole talk, viz., How shall two chemists who differ check each other up? or, come to an agreement with each other? I answer, Where two chemists differ, and the difference is due to working on non-uniform samples, or, on not exactly the same sample, the matter is easily checked up by changing samples. Also, differences due to impurity of the chemicals are not very difficult to check up, either by checking up your chemicals, or by the dummy. Sometimes, it is true, the differences due to impurities may be very abstruse and hidden, and difficult to find; but, by changing chemicals, as we have done a number of times, you can frequently locate the difficulty. Also, still further, where manipulation is at fault, it is not a very difficult thing for the two manipulators to get together and work in the presence of each other, as is frequently done, I understand, in Colorado in assaying. One of my assistants is an old assayer from Colorado, and tells me that, many a time, he and the works' chemist or assayer have worked in the same muffle, side by side, and under the same conditions, so that each could watch the other. Many times, it is not necessary to go as far as this; simply talking over the manipulation will show wherein the difficulty lies. These three difficulties—or, rather, discrepancies due to these three causes—are not very hard to overcome; but, here comes the poser. Suppose that the difference is due to method, and suppose that I say I have used a good method, an approved method, a published method that is recommended, and you say the same in regard to your method—who is going to decide between us? Both of us are, perhaps, a little obstinate, and both of us perfectly right in the position we have taken. If both of us have used regular, well-recommended methods, who is going to decide between us?

Before answering, let us see what is the necessity for a decision. Oftentimes a good many thousand dollars depends on our work. If we make a mistake by using a bad method, or a wrong method, it may mean a good many thousand dollars to somebody whose product we have rejected. He may have to pay the return freight and have the product sent back on his hands; and, as it doesn't pass specifications, he may not be able, without serious loss, to sell it. On the other hand, if the chemist on the other side has used a bad method, we may have to accept inferior material. As you know, more and more every day, large commercial transactions are based on chemical analyses, and differences between chemists may mean thousands of dollars.

We have proposed the following method for overcoming this difficulty. I do not know that it will be approved by the profession, but we do not see any other way out of it, and many chemists and managing men of the different mills whom we have consulted on the matter, have approved the suggestions we have made. The method is simply this: Publish the method we use, and make it a part of the specifications. For example, suppose we are buying spring steel on specifications that it shall not contain over .05 per cent. phosphorus. Now, we will suppose that I use the volumetric method, another man uses the acetate method, and we get different results, who is going to decide between us? We have either got to leave it to some third party, and agree to abide by his decision, or have the same method. Now, everybody knows that it is impossible to run a large commercial laboratory on the acetate method for determining phosphorus; it is too slow; and, also, on the other hand, it is claimed that the volumetric method is not quite so accurate as the acetate method. Perhaps, I shall have something to say about that some other time. But, what are we going to do? We simply say this to the manufacturer: "We want steel for springs that shall not contain over .05 per cent. of phosphorus, and the phosphorus shall be determined in a given way. Whatever you find by this method, that is the amount of phosphorus so far as our transaction goes." We see no other way out of this difficulty. Now, let us see; this is a pretty bold assumption; the chemist of the Pennsylvania Railroad Company assumes to dictate to the profession what methods they shall use. Yes; but only for transactions in which the Pennsylvania Railroad is involved. Use any method you choose for your own work; we do not assume to dictate to you a particle; but, as a means of avoiding the difficulty due to difference of method, we simply say to you, arbitrarily, that this is the method that must be used to determine the phosphorus, for example, in transactions where the Pennsylvania Railroad is involved. If we had a standard method, or if any learned society will give a method which shall be regarded and accepted by chemists as final, we will bow to it instantly; we will adopt anybody's method, if it is applicable. We claim no especial originality; we simply give you the method which we use.

Now let us see what is going to come of this. We prescribe a method which shall be used for determining phosphorus, carbon, silicon, sulphur, or whatever it may be. The method as we use it will give certain results; the other chemist will get the same results, at least we assume that he will. The Pennsylvania Railroad Company puts an upper limit on phosphorus; since phosphorus is injurious to steel, that is to say, the phosphorus must not go above so much; the interest of the railroad company is to have a method that gives as high results as possible so as to keep phosphorus down; the steel works chemist, on the other hand desires that the method in his hands shall show that the steel is low in phosphorus, because if it gets above a certain amount, it is rejected. In other words, the two parties are on opposite sides of the method. It is one's interest to have a method that will cause the rejection of the steel if it is high in phosphorus, and the other wants a method that will make the steel pass. Now, my experience is that in anything where men's pockets are involved on opposite sides of a question, you are pretty apt to get at the truth sooner or later. This is going to bring a criticism of the method of determining phosphorus by the parties in antagonistic interest. Our thought is this: The method we send out to-day is entirely subject to revision; if any one of you finds a hole in it, say so, and the change shall be made just as soon as we can make it, providing your work is confirmed. It is not our method; it is a method to be used to decide certain chemical questions. Our hope is that there will be enough criticism by these parties in antagonistic interest on the various methods put forth, so that sooner or later there will result a method which we will all be willing to accept as standard. It may be that the work of fifty chemists will be required before we get such a method. There is no assumption on our part of superior knowledge; no desire to dictate in any shape or form to the profession; but we have a difficulty to meet, and we cannot see how to get out of it in any other way.

I asked Prof. Langley what he thought of it. He said he thought that some learned society should approve the method. Per contra, a member of the American Chemical Society is reported to have said that he would bitterly oppose any attempt on the part of that Society to sanction any method. He did not think any learned body should sanction a method. On the other hand the agricultural chemists of this country, as I understand it, have done this very thing, namely, they have agreed that in the analysis of fertilizers whatever phosphoric acid is shown by a certain method, which they defined in convention, shall be called "soluble" phosphoric acid, and the amount of phosphoric acid that is shown by another method or modification shall be called the "reverted" phosphoric acid, while the amount of phosphoric acid shown by still another method or modification shall be called "insoluble" phosphoric acid. The method was first proposed five or six years ago; has been modified three or four times, and now nearly all the agricultural chemists in the United States, if I am right, are determining the phosphoric acid in fertilizers by the method adopted by the agricultural chemists in convention. If some convention would take this work off our shoulders, we would be delighted. We assume it only because we do not know how to get along and meet our difficulties any other way.

I shall be very much obliged indeed for any criticisms and suggestions which any of you may have to make on this scheme, and anything you can contribute in any way, shape or form, you will find falls upon very willing ears.

DISCUSSION.

MR. S. A. FORD: I would like to ask Dr. Dudley whether, if he were to form a series of standard methods, it would not hinder any further investigations of other methods; whether it would not have a tendency to stop investigations. Take one thing in my experience, for instance: When I commenced to work on the chemistry of iron, it took us four days to make a manganese determination in steel. Suppose that had been, at that time, the accepted method. and that you had to use that method, how long would it have taken to knock that method on the head and change it? There is the difficulty I find in making standard methods. I know a gentleman in Chicago who, some ten years ago, broached the subject of making standard methods, and I told him that I thought chemistry was a science which was continually advancing, and, if we should make a standard method, we would, in ten years, be away behind the times. Now, again, I know that some commercial chemists laugh at the Eggertz test. Well, if they were in a steel works twenty-four hours, they would find they would have to use the Eggertz test, because it is impossible to make from 125 to 150 analyses a day, as we have to do, by any other method; although we know that the results by this method are, at the best, approximate. It depends very much on the chilling of the steel; a portion of the carbon is invariably burnt up into carbon oxide, and gives no color with nitric acid. What I don't like about standard methods is this: will they not prevent other methods being formed? In a few years, the old standard methods may be proved wrong, and, when a dispute arises, we will have to use the old standard method to decide it, at the same time knowing that the method does not give correct results.

DR. DUDLEY: The question which Mr. Ford raises is one that we thought over a great deal, and this is our explanation to ourselves of this difficulty: First, it is impossible that those who are on opposite sides of a bargain, as I tried to state, and are using the method, will let the method alone; in other words, the stimulus that comes from the commercial side of the question will stimulate the steel works chemist to show that the method is in error, if the method we have prescribed gives a little bit higher result than the method he has been using. If he demonstrates an error in the method, the method would be changed. We would simply put ourselves in the way of progress if we maintained a demonstrated error. Indeed, we have thought that possibly the commercial side of the question as above stated would stimulate growth in methods. If the method we specify for phosphorus gives moderately high results, then every chemist at a steel works wants to prove that the method is in error, because it gives higher results than he wants to have in the steel. So, then, I do not think that the progress of chemistry need be retarded in any sense by the adoption of a standard method. Now, as to the other point raised about the rapidity of the method. The method we prescribe may or may not be applicable to rapid work. Take the method for carbon determination; we are getting a little afraid that the color test will not give concordant results closely enough so that we can use it. We meet the difficulty in another way, namely, the limits of our specifications are wide enough so that the color test, if you aim half way between the limits, will not lead you in error. For example: in boiler plate, the lower limit of carbon is .15 per cent., upper .25. If you aim at .18 or .20 with the color test, and it will not give results within that margin, the color test will have to go. Again, take spring steel; the carbon limits are not below .90 and not above 1.1 per cent.; a limit of .2; the color test ought to work near enough to either side of 1 per cent. to give the carbon within those limits, a tenth either way.

MR. FORD: Take rails for example; the specifications say .45; your inspectors reject .44; their orders are to reject anything below .45. Now, you can't come closer than two points with the color test. The rails are rejected on one point, yet the carbon might have been .46 instead of .44 per cent.

DR. DUDLEY: I would like to say that we give in all our specifications a pattern; something we desire; this is simply to stimulate growth; to spur the manufacturer to do a little better than he is doing; we give a pattern, and then we give a limit some distance below the pattern, and accept the material down to that lower limit. In a number of cases, also, we give an upper limit, beyond which we will not accept the material.

Now, here is the puzzle: The manufacturers aim right square at the lower limit, and the uncertainties of the process are so great that they tumble down about half the time. Let me give you a case. Along in the early days, we put the lower limit for carbon in spiral springs at .90 per cent. A great many men said we were making a serious mistake in putting the carbon so high; they wanted .75 per cent. as a maximum; .60 per cent. was high enough, they said. Without going into ancient history, I will say that we had about a three years' struggle to get the manufacturers up to .90 per cent. They did not allow any margin at all for the burning out of the carbon in the process of manufacture, and bought their steel on specifications that it should not go below .9 carbon. We rejected hundreds and hundreds of springs, simply because the manufacturer aimed at something that would just squeeze through. We think there is no difficulty whatever in filling Pennsylvania Railroad specifications if you will aim at the proper point in the manufacturing; for there is no specification that we know of that has not been made after careful consultation with good manufacturers, and the limits of error necessary, for the uncertainties of commercial work have been allowed for in the limits of the specification. If a manufacturer gets an order from the Pennsylvania Railroad, he will never have a rejection of the material if he will aim between the two limits of the specification; not at the lower limit, so as to just squeeze through, but at the mean between the two.

Mr. Ford: Suppose we aim between .45 and .55. We sometimes have accidents.

Dr. Dudley: You ought not to make us responsible for those.

Mr. Ford: Enough spiegel may not be run in, and then—

Dr. Dudley: You ought to lay that aside for some other fellow.

Mr. Ford: The carbon might be .46, if tested by the combustion method, and the color test might show it as only .44, and the rails would be rejected on that one point.

Dr. Dudley: Now, there is another point on which we have had some interesting experiences. The analysis of a sample in a shipment may not always show the character of the whole shipment. I will give you an illustration. First, I will say, it is our custom to take only one sample of a shipment to represent the whole shipment; we never take any average samples. Our theory is that the material we buy and pay for is all supposed to be good; if it is all good, it is immaterial where the sample comes from; so that one sample fairly represents a shipment. If a manufacturer sends us any poor material, we will take the risk of catching him up on one sample, if he will take the risk of getting his shipment through. Now, here is an illustration: A manufacturer at one time got an order for fifty barrels of the best grade of lard oil. When we got the material, we took a sample; it was very poor No. 2. We rejected the whole lot and he had to pay the return freight. He "kicked" a little, as we say in technical language, and went to the Purchasing Agent, saying that he thought it was very hard; that he had gone over the whole shipment barrel by barrel, and found forty-seven barrels just as good oil as was ever made, and that there was only three barrels that were inferior oil, and he thought that we ought to have taken the forty-seven barrels, and sent back only the three. The Purchasing Agent, who was pretty shrewd, asked him how it happened that there were three barrels of inferior oil. Well, the manufacturer said he only had forty-seven barrels of the grade ordered, and the foreman of the factory had slipped in, unknown at the office, the other three barrels necessary to fill the order. So we say if the shipper is willing to take the chances of having his material rejected, we will take our chances of getting our samples from the inferior part of the shipment. We do not see any reason why, because there is uncertainty in the process, or because you have an accident with the metal, or because you don't get in enough spiegel, or because some of it slops over, we should be held responsible for it.

THE CHAIRMAN: The subject of fixing some standard method is very interesting.

DR. DUDLEY: We purpose, as I have said, to send out a printed proof of this method for determining phosphorus, and the purchasing agent will send it to all parties from whom we buy steel, so that every chemist who will have to use the method will have a chance at it before it is finally issued. The phosphorus method will come out probably in the next two or three weeks, and we hope to follow it soon with the others. I would say for your information that, in doing this, it is, of course, incumbent upon us not to send out a method that will not give uniform results; by which I mean that if I make an analysis with any method, and get a discrepancy in duplex or triplex, there must be something wrong with me or with the method. Now we are trying to do enough work on each method before we send it out, so that, if the directions are fairly well followed, uniform results will be obtained by the method. If there is an element of uncertainty in the method that cannot be covered by explicit directions, then, of course, that will be a bad method to send out. After we have decided on a method we put it into print; then we turn it over to the boys in the laboratory, with no other directions than the printing, and see what they can get out of it. In that way we hope to check up any serious deficiency in the directions. It was just this deficiency in the directions which led to the discrepancy in the phosphorus determination I told you about above. We put in a precaution now on that point to the effect that if you are not careful you may leave enough molybdic acid in the filter to vitiate the result, even though the washings do not show anything by the test. We hope also to get enough criticism from the steel works' chemists on the method when it is sent out, so that any serious discrepancy in results, unless they are simply blunders, will be avoided. It is, of course, an experiment; we do not know how it is going to work out yet, but we simply do not see any other way out of the difficulty.

MR. CAMP: Is the standard method to be exclusively a titration one?

DR. DUDLEY: In phosphorus, yes.

MR. CAMP: Wouldn't it be just as well to weigh the yellow precipitate?

DR. DUDLEY: Well, I hardly know what to say in answer to that. We have not succeeded very well in weighing the yellow precipitate. It is so excessively simple to titrate that we thought it was preferable.

MR. CAMP: It has more manipulation, and the more manipulation there is, the greater the chance of error.

DR. DUDLEY: I am just a little bit inclined to volumetric methods if they can be controlled within reasonable limits. I would like to say in answer to Mr. Camp that I don't think this ought to introduce any difficulty. If you prefer to weigh the yellow precipitate, do so. All that will be required will be this: in case of dispute between you and me, you must use the method as given. If you take the method, and find that by your way you get the same result practically as you do with the method given, and you prefer to weigh, there is not the slightest objection to your doing so, except in case of dispute.

Society adjourned at 10 P.M.

JAMES O. HANDY,
Secretary.

OCTOBER 18TH, 1892.

THE Society met in the parlors of the Academy of Science and Art, on October 18, 1892, at 8 P.M., Mr. Alfred E. Hunt, President, in the chair; Mr. R. N. Clark, Secretary; forty-nine members were present. The minutes of the last meeting were read and approved. The following were elected members of the Society:

Walter H. Jackson, W. G. Muse, George F. Greenwood, Maurice Coster, Horace W. Lash, Wm. H. Hays, Reuben Miller, Jr., H. H. McClintic, Wm. A. Herron, Chris. H. Snyder and W. D. Corcoran.

The President called attention to the request of the American Society of Civil Engineers that the members of the Society of Engineers of Western Pennsylvania are asked to contribute papers on the special list of papers suggested in their original circular, and that one member of this Society should be appointed to solicit them.

On motion, Mr. W. G. Wilkins was appointed such solicitor.

Mr. George Hunt Hutchinson then read his paper on

MILL BUILDING CONSTRUCTION WITH DETAILS FROM PRACTICE.

THE writer wishes to present, for your consideration and discussion, a brief statement of the more important conditions which should govern the design and construction of modern mill buildings. The general dimensions of the building, in any given case, are determined by the use to which it is to be put, while the sections of its members depend upon the strains due to the various loads it is called upon to carry.

General Types of Buildings.—There are three general types in common use.

The first has a rigid iron frame throughout. Each transverse bent is made up as follows: An iron roof truss is supported at its ends by iron columns firmly anchored to masonry piers. The bent is made rigid by transverse bracing, consisting of knee-braces in intermediate bents and vertical bracing in the end bents between columns. In the sides of the building vertical bracing is provided between the columns and lateral bracing in planes of top and bottom chords of roof trusses. When long panels are desired, the alternate roof trusses can be supported on longitudinal girders, or trusses, running between the columns. At the ends of the building one or more gable columns should be introduced to shorten the unsupported length of transverse vertical bracing. The sides and ends may be covered with corrugated iron sheeting supported on suitable frame work.

The second type differs from the first in having thin curtain walls of brick built up to the top of columns on all sides of the building. These walls carry no vertical loads, but must do the work of the vertical bracing between the columns in the sides and ends. This bracing is, therefore, omitted; but there should be in the sides of the building a top strut running from column to column, and connecting to the inner flanges, in order to clear the brick wall. If desired, the top strut may connect on the centre line of columns, in which case the wall must be built around it.

Buildings of the first type of construction are suitable to withstand the action of the heaviest jib and travelling cranes, since they are able to resist large horizontal forces, as well as the usual vertical loads. The second type, while suitable for buildings of heavy construction, and especially adapted for machine shops, are not so well fitted to endure the action of cranes of large capacity, particularly, if the building be high and narrow. If for any reason the brick walls are considered essential and it is desired to secure the greatest strength and rigidity possible, regardless of expense, a combination of these two types would give a result which could not be surpassed.

The last type of construction to be mentioned, is well adapted to buildings intended for heavy machinery and light jib cranes, and may be used for suspended travelling cranes. In this third type there are no iron columns. The walls of the building are brick and the roof trusses rest directly upon them. Additional rigidity can be secured by the introduction of iron columns at the four corners of the building. These columns should be well anchored to the foundations. The roof trusses should be braced in the planes of the top and bottom chords. If there are no jib cranes for which to provide, the bottom chord bracing may be put in alternate panels only; in which case, however, the top chord bracing should be in every panel to keep the walls in line.

Classification of Forces and Their Action.—At this point it may be well to state, at some length, the character of the loads and forces, which come upon a mill building, to examine the action of these loads upon the structure and determine the provisions necessary to withstand them. To ascertain the amount of the dead and live loads and find the stresses resulting therefrom, involves an important problem by no means easy of solution, as will be readily seen from what follows. For convenience in treatment, the forces to be considered will be grouped under two heads,—vertical loads or forces and horizontal forces.

Vertical Loads or Forces.—Under this head come the weight of the iron frame and its covering; the weight of snow; the vertical component of the wind pressure; travelling cranes and their lifted loads; the weight of pipes, machinery and shafting; the action of driving belts; and such additional loads as may arise in individual cases. The importance of considering the action of stresses due to cranes is evident, when it is remembered that there are now in use jib cranes of varying capacity up to fifty tons and travelling cranes up to one hundred tons and even one hundred and fifty tons lifting power. These cranes produce not only direct stresses in main members and bracing, but also heavy bending moments in the columns and alternating stresses in various members throughout the building. The action of long lines of shafting, with their driving pulleys and belts, and of hydraulic and steam machinery, frequently contribute a large share to the sum of uncertain and, too often, unconsidered stresses to which a mill building is subject. While it is impossible to determine these stresses with accuracy, it is nevertheless, necessary to make allowance for them. And the proper way to deal with them is to assume, as nearly as may be, an equivalent uniform load and a single concentrated load, which may be applied at will to any member of the structure. These vertical loads vary so in amount, depending on the span and type of building, that it is impossible to give an equivalent load per square foot which could be used indiscriminately. It must be ascertained independently for individual cases. The action of the vertical loads on the structure, when once they or their equivalents have been computed, is readily determined and need not be further discussed.

Horizontal Forces.—Under this head come the horizontal component of the wind pressure and the thrusts from the travelling cranes, jib cranes, belts, etc. The horizontal action due to the cranes and wind is of considerable importance and is an element which does not receive the attention it merits. Fortunately, however, the maximum wind and crane loads occur but rarely and the probability of their occurring simultaneously is so small that, if we proportion each member for the larger stress, we may disregard the smaller. Horizontal forces act on a building, tending to move the structure horizontally and to overturn it as a whole. As a result, stresses are produced in the individual members throughout the building. Friction and the weight of the structure are usually sufficient to counteract the tendency to movement and we need consider the action on the individual members alone. This action can be best shown by means of a stress diagram: see Plate I., Fig. 1.

The following conditions will be assumed to illustrate the method of analysis:

	Ft.	in.
Height to centre line of bottom chord of roof truss,	42	6
Rise of roof truss,	16	0
Height of ventilator,	7	6
Total height of building,	66	0
Span of building, centre to centre of columns,	64	0

The building consists of a rigid iron framework throughout.

The wind-force will be taken at the low value of 20 pounds per square foot. The wind will be assumed to blow in a horizontal direction in all cases. The vertical dead load, acting on roof, is taken at 25 pounds per square foot, horizontal projection; with an additional vertical loading, acting on columns, of 25 pounds per square foot, horizontal projection. These two loads cover the weight of the iron framework of the structure, the roof load being the weight of the trusses and their covering, and the additional column load being the weight of the columns and their bracing. The permanent loads only have been selected for analysis, so that the liability to alternating wind stresses in the roof trusses may be fully shown. While the estimate for dead weights is made to more than cover their amount in general practice, no extreme data have been used.

Were we designing the building for construction, it would be necessary to assume, in addition to those already mentioned, vertical loads something like the following:

For snow, 10 pounds per square foot.

For equivalent uniform load, 10 to 15 pounds per square foot.

For concentrated load, 10,000 pounds.

The equivalent uniform and concentrated loads above mentioned are those referred to in the paragraph relating to vertical forces and cover the action of shafting or other indeterminate loads, which would affect a building, according to the particular purposes for which it is intended. If cranes were to be put in the building, their action would also have to be considered. For the present discussion these loads are not included in the analysis.

The diagrams on Plate I. show:

CASE 1.—The stresses in roof-trusses, columns, and knee-braces, for wind acting on one entire side of the building and roof, for columns hinged at base:

CASE 2.—The same conditions of loading, for columns rigidly fixed at base by anchor bolts:

CASE 3.—The stresses in roof-trusses and columns for permanent dead-load:

CASE 4.—The stresses in roof-trusses for wind on roof only. The roof-trusses rest on walls, and are anchored at both ends:

For Cases 1, 2, and 4, the entire horizontal force of 20 pounds per square foot is considered on the vertical projection of all surfaces acted on by the wind. The vertical component is disregarded.

CASE 5.—The stresses in roof-trusses, columns, and knee-braces, for wind acting on one entire side of building and roof, for columns rigidly fixed at base by anchor bolts.

For Case 5 only, the analysis is made for the resultant normal pressure on the several surfaces acted on by the wind.

It is customary to consider the resultant wind-pressure as acting normally to the roof-surface. We have, however, in the present instance, for the purpose of comparison, found the stresses resulting from the normal pressure, and also from the full horizontal force of the wind acting on the vertical projection of the entire building. In the latter case, there will be no vertical component. When the resultant normal pressure is treated, as in Case 5, the wind-stresses may be found in one diagram for this normal pressure, or the stresses for the horizontal and vertical components may be found separately and combined for total wind-stresses.

Cases 2 and 5 show a comparison of the effects of the full horizontal force of the wind, and of the resultant normal pressure. All the conditions in these two cases are the same, except that of the direction of the resultant wind-pressure.

In high buildings, especially with ventilators, on account of the small proportion of exposed roof area, the difference in the resulting stresses is not very marked; the normal pressure giving in most members the smaller results. According to Unwin the formula $P = F \sin. i^{1.84 \cos. i - 1}$,* deduced from Hutton's experiments, expresses the relation, for wind blowing horizontally, between the horizontal force of the wind on a vertical plane and the normal pressure on a surface inclined to the horizon. In this formula,

F = the horizontal force of the wind on one square foot of a vertical plane, placed at right angles to the direction of the wind.

P = the resultant normal pressure of the wind on one square foot of a roof surface inclined at an angle i to the horizon.

The following table gives values of P derived from the above formula, for F, equal to 10 pounds per square foot, and for various inclinations of roof surface. Intermediate values of P can be found by interpolation.†

For roofs having a steeper pitch than 60°, the resultant normal pressure may be taken equal to the full horizontal force of the wind.

---

* For a clear explanation of this formula, see Greene's *Graphical Analysis of Roof Trusses*, published by John Wiley & Sons.

† A diagram giving the values of P, derived from the above formula, is contained in Howe's "Diagrams, Formulas and Tables for Bridge Engineers and Architects," reprinted from *Mechanics*, 1890, for private circulation.

i = Inclination of roof-surface to the horizon.	P = Resultant normal wind-pressure per square foot of roof surface, in pounds.	i = ditto.	P = ditto.
5 degrees.	1.3	35 deg.	7.52
10	2.4	40	8.35
15	3.5	45	9.02
20	4.57	50	9.52
25	5,62	55	9.9
30	6.62	60	10.0

The intensity of the wind stresses throughout the roof truss is dependent, other conditions remaining constant, upon the ratio of the total length of the column to the length of the portion above the foot of the knee brace. The greater this ratio, the greater will be the stresses.

Although the knee braces have been assumed of greater depth than will ordinarily occur in practice, an examination of the diagrams will show for Cases 1 and 2, and even for Case 5, that the wind stresses in some of the principal members on the windward side of the roof trusses are equal to the dead load stresses, while on the leeward side the stresses are of opposite character and of even greater intensity. Note especially the stresses in the top and bottom chords of the roof trusses, the two main diagonal ties running to the ridge, in the knee braces and the adjoining diagonals.

The analysis for the horizontal action of crane loads, is similar to that given for wind, except that it is simpler, as there are fewer loads to deal with; therefore it is not necessary to give the analysis for this class of forces, but in practice, the maximum stresses arising from either wind or crane loads, should be provided for; considering such part of the crane load as may be deemed proper for the bent under consideration.

Systems of Bracing.—Mill buildings should be thoroughly braced to resist the action of horizontal forces and to keep them properly lined up. For this purpose longitudinal and transverse bracing in a vertical plane and lateral bracing in a horizontal plane are required.

Ventilator Bracing.—There should be a strut running from the top of each ventilator post to the ridge of the main roof truss, to prevent distortion of the ventilator frame. Vertical longitudinal bracing is needed in the sides, and at the centre line of ventilators, to keep the frames upright and parallel. When swinging windows are placed in sides of ventilators the bracing there must be omitted, and replaced by stiff bracing, fastened to the ventilator purlins.

Rafter Bracing.—The purpose of this bracing is to keep the trusses from overturning, and it should be strong enough to resist the action of the wind on the gable end of the roof. This bracing may be omitted in alternate bays.

Bottom Chord Bracing.—The purpose of this bracing is to keep the columns in line and to distribute concentrated loads.

Vertical Bracing Between Columns should be provided to transmit to the foundations, all stresses due to horizontal forces. The greater the depth of this bracing for given length of column, the less will be the resulting stresses in the columns, trusses, and in the bracing itself. Usually, it may come down to a point eight to ten feet from the ground. The bracing in the sides takes the longitudinal thrust of the wind and crane loads, and the bracing in the ends resists the part of the transverse action of those loads, received through the bottom chord bracing.

It will be well to consider, in a general way, the action of the wind and crane loads on these systems of bracing in the sides and ends of a building. Both of these classes of forces, if allowed, would act on the structure in the same general manner; but the different conditions under which they are applied warrant the provision of separate systems of bracing to transmit them to the foundations. It is impracticable, in long buildings, to make the bottom chord bracing heavy enough to carry the cumulative wind loads from the successive panels to the ends of buildings. It is, therefore, necessary that each intermediate bent should resist its own wind load and a portion of the distributed crane loads. To do this knee braces are provided at each bent running from bottom chord to column. The conditions of head room allow the knee brace to extend down the column a short distance only, and the resulting stresses from the wind, as has been shown in the diagram, are large in the trusses and columns and in the knee braces themselves. These members should, therefore, be made abundantly strong to resist these stresses. The uniform wind loads having been provided for, the crane loads and other local loading only remain to be considered. The bottom chord bracing must take these loads and distribute them to the adjacent bents, transmitting the residue, if any, to the vertical bracing in the ends of the building. This is an economical arrangement, since the maximum crane stresses occur simultaneously in two or three panels only, and, consequently, are not seriously cumulative. Crane stresses are occurring constantly, and their repeated action would be a severe test upon the rigidity of the structure if each bent were required to resist the stresses from loads in the adjacent panels. On the other hand, large wind loads occur rarely and the individual bent can, without injury, withstand them. We do not mean to say that in practice the wind will be taken by the individual bents and the crane loads by the bottom chord bracing, because there is, necessarily, great ambiguity in the distribution of these loads, but the bottom chord bracing will have served its purpose if it relieve individual bents of the racking effect of concentrated loads.

Action of Horizontal Forces on the Columns.—The column in resisting horizontal forces acts as a beam and when rigidly anchored it may be considered as fixed in direction at the base, but when not properly anchored, it must be considered as hinged at the base; or in other words free to rock on the pier. Theoretically we might treat the column as fixed in direction at the top also, when rigidly connected to the roof-truss, but as we are not at all sure of realizing a sufficient degree of rigidity at this point it will be safer to consider the column as simply supported at the top in all cases. In reality there would be but little gain in fixing the column in direction at the top, as it must be done at the expense of resulting bending moment in the roof-truss. The column then in resisting horizontal forces, will, when rigidly anchored at the base, act as a beam fixed at one end and supported at the other, but when not properly anchored, it will act as a simple beam supported at both ends. The anchor bolts, then, in fixing the column, have to resist the bending moment at the base; or in other words they must counteract the tendency of the column to rock on the pier. They must also resist the horizontal thrusts not overcome by friction. The rocking tendency is influenced by the width of column base and by the depth of vertical bracing. The lower the bracing extends, the smaller will be the bending moment at the foot of the column and the pull on the anchor bolts. If the bracing were to extend the entire length of the column, the bending moment at the base and the pull on the anchor bolts would be reduced to zero. In practice the bracing can run down the column a short distance only, and bending moments will occur at the foot of the bracing and at the base of the column. As a result, the column, acting as a beam, will be distorted, and a point of contra-flexure will occur in the unsupported portion. If the anchor bolts be removed, the case will be that of a beam supported at both ends. The point of contra-flexure will then disappear and the bending will be largely increased.

From this discussion we observe the following important facts: Deep bracing and strong anchorage reduce the strains in the columns, roof trusses and the bracing itself. In designing the column the moments at the base and at the connection of the bracing should be provided for in the section. The anchor bolts should be made of sufficient section to thoroughly fix the column at its base.

Types of Roof Trusses.—While there are several types of roof trusses well adapted to mill buildings, probably the one in most common use is the "French" roof truss. This truss is simple in design and suitable for either light or heavy construction. For several other types frequently used, see Plates I.–V. Whatever the type, it is always best to make the bottom chord of the truss straight, except when the conditions of head room require it to be raised.

Sections and Details.—Irrespective of the type of roof truss there are two radically different styles of construction which may be used: one is pin-connected and the other is riveted work throughout. While there is a large variety of sections from which to choose in the riveted style, the T-shaped section is most commonly used. This is composed of two angles placed back to back, with or without a single web plate between. This form of section may be used for both tension and compression members. A single flat bar may be used for the smaller tension members. Roof trusses built in this manner lack somewhat in rigidity and lateral stiffness.

In the all-iron type of building—that is, where the roof trusses rest directly upon the column—rigidity and stiffness are important factors, and the construction shown in Fig. 1, Plate II., is frequently used with satisfactory results. In this case the compression members are made up of two channel bars turned back to back and latticed, forming a box section. For members subject to tension only loop-eye rods are used. The truss is pin connected throughout. Upon examination of the figure, it will be observed that all the connections are readily made and that the details are simple in design. But little riveting is required, the shop work can be easily and quickly turned out, and the building rapidly erected in the field. The bottom chords of the trusses will stand heavy loads in bending and afford especially good connections for jib cranes and lateral bracing. The roof trusses have such strength and rigidity as to impart stiffness to the entire building. This style of construction is well adapted to both light and heavy buildings.

There is another style of construction suitable for heavy work only, which is quite as rigid as the one just mentioned. In this the top and bottom chords are of box sections and made up of plates and angles instead of channels. The web members have an I-shape and are made up of four angles placed back to back in pairs and latticed. All the truss members are stiff, and may have either pin or riveted connections.

When these built sections are used with riveted connections, the shop work is somewhat less expensive than for the pin-connected channel construction. When, however, pin-connections are to be used for both styles of construction, the channel sections require less shop work and afford simpler details than the built sections.

Roof columns and light crane columns may be constructed of Z-bars, channels or plates and angles, but for simplicity of details the channel columns are to be preferred to all others. For heavy crane columns, however, it may be necessary to use plates and angles to secure a column of sufficient width.

Referring again to roof trusses, whatever style of construction is used, the following points should be well considered: The trusses must be designed to withstand the maximum stresses produced by any possible combination of vertical and horizontal loads or by the vertical loads alone. The bottom chords of trusses should be made stiff enough to take both the tension and compression to which they are subjected as well as the bending from local loads. They must afford a convenient connection for the longitudinal struts in the bottom chord bracing; in fact, they are themselves important struts in that system of bracing.

Columns and Girders for Travelling Cranes.—Track girders for light travelling cranes may be supported on brackets on the roof columns, but as this arrangement gives an eccentric loading, it is better for heavy cranes to provide a separate column to carry this load. To prevent unequal settlement, the crane columns and roof columns should stand on the same base. The connection between these columns should be rigid, so that, as far as possible, they may act as one member. The girders must be thoroughly connected to the crane columns and braced laterally to the roof columns. Knee braces from crane columns to girders give longitudinal stiffness. For track girders I-beams or plate girders are preferable to lattice girders. Long heavy girders should have the box section, with diaphragms at intervals between the webs. See Plate II., Fig. 5.

Rails for Track.—In order that the alignment of the track be true, it is best to drill the holes for rail fastening, with girders in position and the rails lined up. Oak packing is frequently put under the rails and at the girder seats to insure smoothness in the running of the cranes. Several rail sections, with method of fastening are shown on Plate I. Details of crane columns and track girders are shown on Plate II., Fig. 5.

Splices and Connections.—Members subject to tension and compression should have their splices and end connections made to resist the maximum stresses of both kinds. When bending stresses occur, they should be considered in designing the splices; this necessitates both flange and web splices. As an example, the splices in the bottom chord of the roof trusses should be able to develop the full strength of that member in tension, compression and bending. The splice at the ridge of the roof truss has to resist the horizontal thrust from the rafters and any vertical shear which may arise from unsymmetrical loading. It is well to remember that the wind is an important case of unsymmetrical loading. In addition, provision must be made to introduce into the rafters the stresses from the web members which meet at this point.

The connection of roof trusses to columns has to resist large horizontal and vertical stresses. In the analysis given for wind in Case 1, shown on Plate I., the horizontal shear on the leeward side is 40,000 pounds and the upward pull from the roof truss is 22,000 pounds, which gives a resultant of about 46,000 pounds to be resisted by the connection. This resultant is net, that is, exclusive of the 25,000 pounds dead load from the rafter. On the windward side there occurs a resultant of 55,000 pounds from wind alone, or 70,000 pounds from wind and dead loads combined. The style of connection determines whether, on the windward side the larger or the smaller resultant should be provided for.

Column Bases.—The gusset plates and bracket angles at the foot of the column have several important duties to perform, which require them to be thoroughly connected thereto. They assist in distributing the column loads to the base plate; they give stability to the column by broadening its base, and furnish a connection for the anchor bolts. The base plate should be large and strong enough to distribute to the pier the loads received from the enlarged column base. The anchor bolts should pass through the bracket angle and, at the same time, be placed as far from the centre line of the column as practicable. They should be anchored in the masonry sufficiently to develop the full strength of the bolts. Large anchor bolts should be built deeply into the masonry; small anchor bolts, however, may be roughened or split and wedged and set in drilled holes, with hydraulic cement, lead, or sulphur. See Plate I., Fig. 1.

Piers.—The masonry piers for supporting the columns should be of sufficient dimensions, weight and strength to resist the vertical loads, the horizontal thrust and the overturning action from the column. They should extend well below the frost line and reach a firm bottom. If necessary, concrete or piles may be used to secure a suitable foundation. To secure a uniform distribution of the column loads, the piers should be provided with a cap consisting of either a single stone or a cast-iron plate. If the stone be used, it should be large enough to give a margin all around the column base equal to one-third of its thickness, which latter should be at least one-third its largest dimension.

Purlins.—For roof purlins, angles, Z-bars or I-beams may be used, with or without trussing. Single channels do not have sufficient lateral stiffness to make good purlins. Several styles of trussed purlins are shown on Plate VI., Figs. 1, 2 and 3. Fig. 2 shows a simple and desirable style of trussing, using star shapes and flat bars. Fig. 1 shows a good section for a long and heavily loaded purlin, made up of two channels, forming a box section and held together by tie plates. Loop-eye rods are used in the trussing, which is pin-connected. Care should always be taken to turn the shape used as a purlin so as to secure the greatest vertical depth. Plate VI., Figs. 5 and 6, show respectively the correct and incorrect method of placing the purlin upon the rafter. Fig. 5 also shows the method of bracing I-beam purlins to rafter by the use of bent plates. Purlins act as longitudinal struts in the system of rafter bracing. At the ridge, under ventilators, no purlins are required. It is, therefore, necessary to have a ridge strut to complete this system of bracing. Long purlins are liable to sag in the plane of the roof. Plate VI., Fig. 4, shows a method of holding them in place by tie rods between the purlins and running from ridge to eaves.

Expansion.—Roof trusses resting on brick walls should have one end free for expansion. A wall plate, for a sliding surface, should be used for spans to about 75 feet and rollers should be provided for larger spans. Roof trusses resting on columns must be attached rigidly thereto, no provision being made for expansion. It is customary to introduce an expansion panel in long buildings, at points 100 to 150 feet apart, to provide for longitudinal expansion.

Corrugated Iron Sheeting.—The use of corrugated sheeting to close the sides and ends of buildings where brick walls are not present, has already been mentioned. It is used, as well, for a roof covering and has the advantage of being cheap, light in weight, and incombustible. Furthermore, it is quickly and easily put on and readily renewed. Its most objectionable quality is its liability to rust. This can be retarded if the sheeting be kept well painted on both sides and still further by using galvanized sheeting. Common weights of sheeting for roof are Nos. 18 and 20, and for sides of building Nos. 20 and 22. For No. 20 sheeting, purlins should be spaced not over 6 feet centres, and preferably less than this.

Lighting and Ventilation.—Ample provision should be made for lighting and ventilation. Windows with swinging, sliding or fixed sash may be introduced in the sides and ends of the building and in the sides of ventilators. Louvres for ventilation may replace the windows where desired. Additional lighting surface can be obtained by the introduction of skylights on the main and ventilator roofs. In the sides of a building which has brick walls it is a simple matter to provide openings for windows and doors. In buildings with no brick walls framework between the columns is necessary to support the doors, windows and sheeting. This framework may be all timber, all iron or a combination of iron and timber. The framework entirely of timber is suitable for light buildings only. The framework entirely of iron is required in buildings where combustible material is prohibited. In the latter the bracing and framework supporting the windows are of structural iron and the window frames and sash are galvanized iron. An all-iron frame for a gable is shown on Plate VI., Fig. 7. The combination timber and iron framework is suitable for all buildings where absolute fire-proof construction is not needed. In this case we have a complete system of iron bracing between the columns. The main girts are of iron, but the girts and posts supporting the windows and corrugated iron are of timber and are introduced between the main girts wherever needed. This style of construction is shown on Plate II. In the all-iron type of construction the bracing between the columns is frequently stopped 8 to 12 feet above the ground. The space below the bracing may be left entirely open or closed with corrugated sheeting and swinging, sliding or lifting doors.

General Conclusions.—In the design of mill buildings of the present day it seems to the writer that there are certain stresses incident to the everyday operation of the mill which do not receive proper recognition. We can with profit make a comparison of the design of the mill building with that of the railroad bridge, since in general the same principles apply to both. In the latter provision is made for certain secondary stresses, such as impact, wind and centrifugal force. In addition, the probable increase in loading during the life of a structure is considered. In like manner, we have in the mill certain secondary stresses due to the actión of cranes and wind equal in importance to those above. The question of the future increase in loading of the mill building should be given the same attention as it receives in bridge construction. We have already considered the action of these secondary forces upon the building and have shown that they must be provided for by the use of liberal sections and suitable bracing in order to secure rigidity. The uncertainty respecting the increase in loading which future conditions may dictate for the structure, is especially marked in the case of extensive plants, in which, on account of rapid development, radical changes are not unusual. While the writer would not be understood to advocate the introduction of material in members throughout the structure regardless of their present or probable future requirements, he does hold that in the long run it is economy, in the case of permanent structures, to provide not only for loads which it is known will occur, but for those which experience teaches are within reasonable range of possibility. The objection to all this is that it costs money, but small first cost is not always true economy. A cheap building will in time cost enough for repairs and remodelling to make it an expensive investment. Furthermore, delays in the operations of the mill, which are liable to result from weak and faulty construction, are, at best, expensive drawbacks and frequently are far-reaching in their consequences.

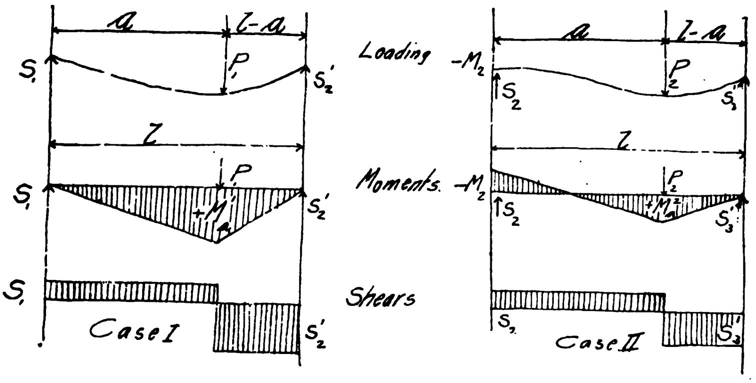
A principle well worth noting, and one which argues strongly in favor of designing for liberal loads, may be stated in this connection: After the material has been provided to make a structure strong enough to carry a moderate loading, the introduction of a reasonable amount of extra material will give an increase in carrying capacity which is entirely out of proportion to the expense incurred for such increase.

In conclusion, we may say that the three most essential factors in the design and construction of mill buildings named in the order of their importance are Strength, Simplicity and Economy.

The accompanying plates represent several types of structures recently erected in the vicinity of Pittsburg. The author is indebted to the courtesy of the management of the Keystone Bridge Works for the drawings of work, constructed by them, from which the details were selected. The plates are largely self-explanatory and need no further description in the text.

APPENDIX.

Analysis for Wind.—The following is a brief statement of the conditions assumed and the methods employed in the analysis for wind stresses. The reactions, shears and moments are determined analytically; the resulting stresses in the knee-braces and roof-trusses are determined graphically as shown on Plate 1.



Case I. in the accompanying sketch represents the loads, shears and moments for either column of a bent, hinged at base and acting as a simple beam supported at both ends.

Case II. represents the loads, shears and moments for either column of a bent, rigidly fixed in direction at base by anchor bolts and acting as a beam fixed at one end and supported at the other.

NOMENCLATURE.

Known Terms:

b = span of roof.

l = length of beam or column.

a = distance from base of column to foot of knee-brace or to the point of application of the load P_1 or P_2.

$k = \frac{a}{l}$.

S_1 = the horizontal component of reaction or shear at base of either column of a bent when hinged at base. Case I.

S_2 = horizontal component of reaction or shear at base of column when fixed at base. Case II.

$S_1 = S_2$.

S_2^1 = shear at top of column when hinged at base.

Unknown Terms:

S_3^1 = shear at top of column when fixed at base.

P_1 = horizontal thrust or load at foot of knee-brace, due to the leverage action of the column when hinged at the base.

P_2 = horizontal thrust or load at foot of knee-brace, due to the leverage action of the column when fixed at the base.

M_2 = bending moment at the foot of column when fixed at base.

M_x^1 = moment at any section of column distant x from the base, for column hinged at base.

M_x^2 = moment at any section of column distant x from the base, for column fixed at base.

M_a^1 = moment in column at foot of knee-brace = maximum bending moment, for column hinged at the base.

M_a^2 = moment in column at foot of knee-brace = maximum bending moment, for column fixed at the base.

m = the sum of the moments of the horizontal wind-loads above any point in the axis of either column taken as the centre of moments; note especially that this is a variable quantity, its value depending upon the height of the point taken as the centre of moments.

V_1 = the vertical reaction at either column due to the overturning action of the horizontal component of the wind-pressure on one entire side of building and roof, for columns hinged at the base.

V_2 = the vertical reaction at either column due to the overturning action of the horizontal component of the wind-pressure on one entire side of building and roof, for columns fixed at base.

V_1 and V_2 are independent of the vertical reactions resulting from the vertical components of the wind-pressure.

Formulas—Applicable to Either Column of a Bent.

Case I.—For columns hinged at the base and considered as a simple beam supported at both ends.

$$S_2^1 = \frac{S_1 a}{l-a}, \qquad \text{(1).}$$

$$P_1 = \frac{S_1 l}{l-a}, \qquad \text{(2).}$$

$$P_1 = \frac{S_2^1 l}{a}, \qquad \text{(2a).}$$

$$M_x^1 = S_1 x, \qquad x \leqq a \qquad \text{(3).}$$

$$M_x^1 = S_1 x - P_1 (x-a), \qquad x > a \qquad \text{(3a).}$$

$$M_x^1 = S_2^1 (l-x), \qquad x > a \qquad \text{(3b).}$$

$$M_a^1 = S_1 a, \qquad \text{(4).}$$

$$M_a^1 = S_2^1 (l-a), \qquad \text{(4a).}$$

$$V_1 = \frac{m}{b} \text{ only when } x = o, \qquad \text{(5).}$$

$$V_1 = \frac{m}{b} + \frac{2S_2^1 (l-x) - 2P_1 (a-x)}{b}, \qquad x < a \qquad \text{(5a).}$$

$$V_1 = \frac{m}{b} + \frac{2S_2^1(l-a)}{b}, \quad . \quad . \quad x = a \quad . \quad . \quad . \quad . \quad (5b).$$

$$V_1 = \frac{m}{b} + \frac{2S_2^1 l - 2P_1 a}{b}, \quad . \quad . \quad x = o \quad . \quad . \quad . \quad . \quad (5c).$$

Since $2S_2^1 l = 2Pa$, the second term becomes zero, which is the condition when the centre of moments is taken at the point of zero bending moment in the column, and equation (5c.) reduces to $V_1 = \frac{m}{b}$, which is identical with equation (5).

CASE II.—For columns rigidly fixed at the base and considered as a beam fixed at one end (the base) and supported at the other (the top).*

$$M_2 = \frac{-P_2 l}{2}[2k - 3k^2 + k^3], \quad . \quad . \quad . \quad . \quad . \quad . \quad Eq.\ (6),$$

or taking moments about the base of column we may write in terms of P_2 and S_3^1,

$$M_2 = -P_2 a + S_3^1 l, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6a).$$

$$M_x^2 = -M_2 + S_2 x, \quad . \quad . \quad x \leqq a \quad . \quad . \quad . \quad . \quad (7).$$

$$M_x^2 = -M_2 + S_2 x - P_2(x-a), \quad . \quad x > \quad . \quad . \quad (7a).$$

At point of contreflexure, $M_x^2 = -M_2 + S_2 x = o$ and

$x = \frac{M_2}{S_2}$ = distance base of column to point of contreflexure, (7b),

or independently in terms of S_3^1 we may write

$$M_x^2 = S_3^1(l-x), \quad . \quad . \quad x \geqq a \quad . \quad . \quad . \quad . \quad . \quad (7c).$$

$$M_x^2 = S_3^1(l-x) - P_2(a-x), \quad x < a \quad . \quad . \quad . \quad . \quad (7d).$$

$$M_a^2 = -M_2 + S_2 a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

$$M_a^2 = S_3^1(l-a), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8a).$$

$$S_2 = \frac{P_2}{2}[2k - 3k^2 + k^3] + P_2(1-k), \quad . \quad . \quad . \quad . \quad (9).*$$

---

* Equations (6), (9), and (10), are expressions for a beam fixed at one end and supported at the other, referred to the origin of co-ordinates at a point over the support at the fixed end, and can be found in text-books on the subject. Valuable tables for the easy solution of these equations are given in Howe's *The Continuous Girder.—Eng. News Pub. Co.*

The other equations, under Case II., and all the equations for Case I., can be written directly from a consideration of the action of the external forces, and require no extended demonstration.

Rail S... Plate I.

with ...ers' Soc. W Penna

...son on Mill Building Construction

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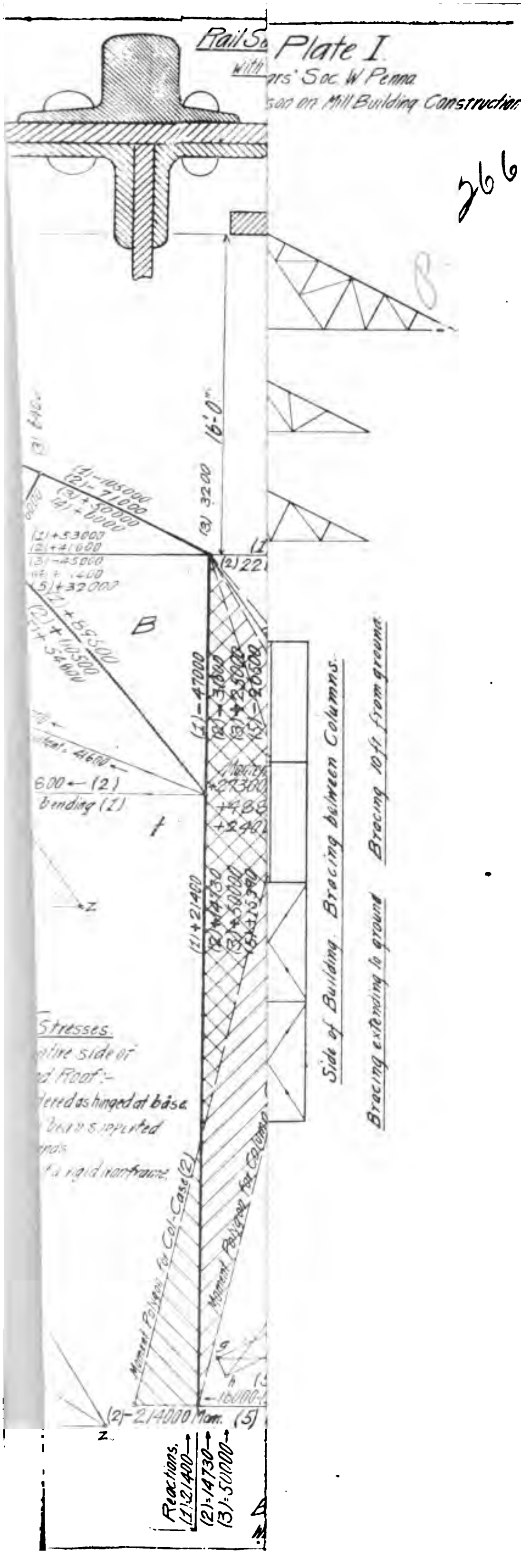

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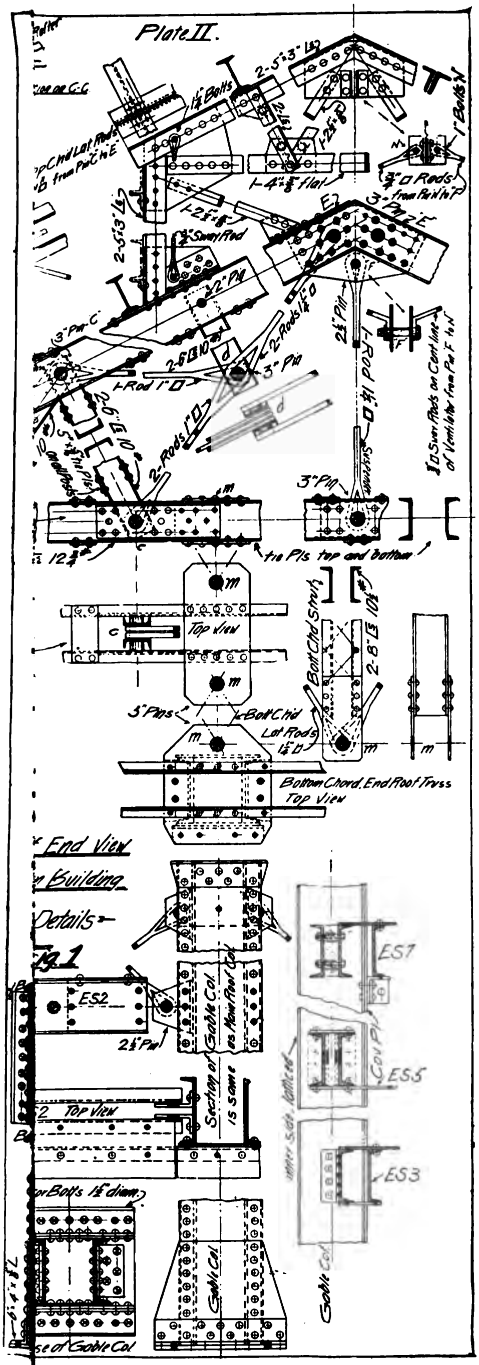


Plate II.

Rafter
Line on C-C
Top Chd Lat Rods from Pin C to E
1¼" Bolts
2-5"×3" Ls
2-5"×3" Ls
1-2½"×⅝"
1" Bolts
N
¾" □ Rods from Pin N to F
1-4"×⅝" flat
1-2½"×⅝"
E
3" Pin E
⅞" Sway Rod
2" Pin
3" Pin C
2-Rods 1¼" □
2½" Pin
1-Rod 1¼" □
F
2-6" [10#
d
3" Pin
1-Rod 1" □
2-Rods 1" □
2-Rods
2-6" [10#
5"×⅜" tie Pls
10#
Suspender
3" Pin
⅝" □ Sway Rods on Cent line of Ventilator from Pin F to N
12¾#
c
m
tie Pls top and bottom
Top View
c
m
m
3" Pins
Bott Chd Lat Rods 1¼" □
Bott Chd Strut
2-8" [10½#
m
m
m
Bottom Chord, End Roof Truss Top View
End View
Building
Details
Fig. 1
B
ES2
2½" Pin
Section of Gable Col. as Main Roof Col.
is same
ES7
ES5
Cov Pl.
inner side latticed
ES3
2 Top View
B
Anchor Bolts 1½" diam.
6"×4"×⅜" L
Base of Gable Col.
Gable Col.
Gable Col.

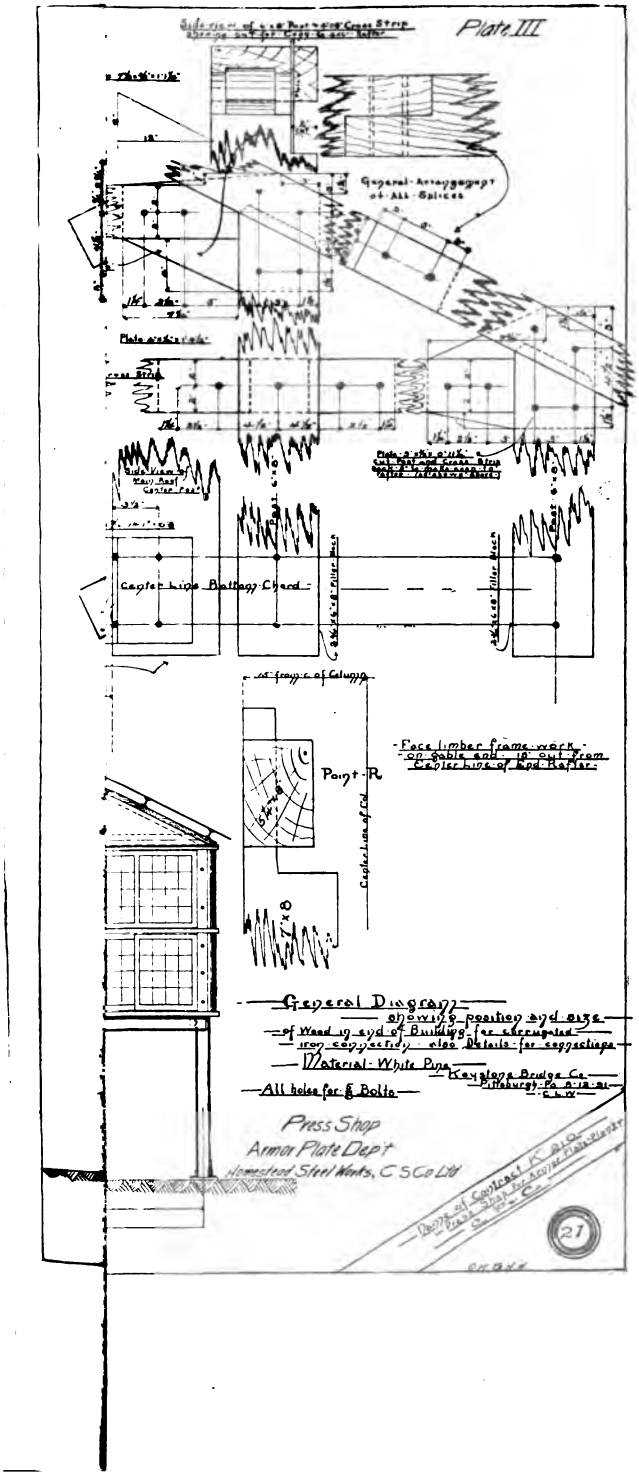
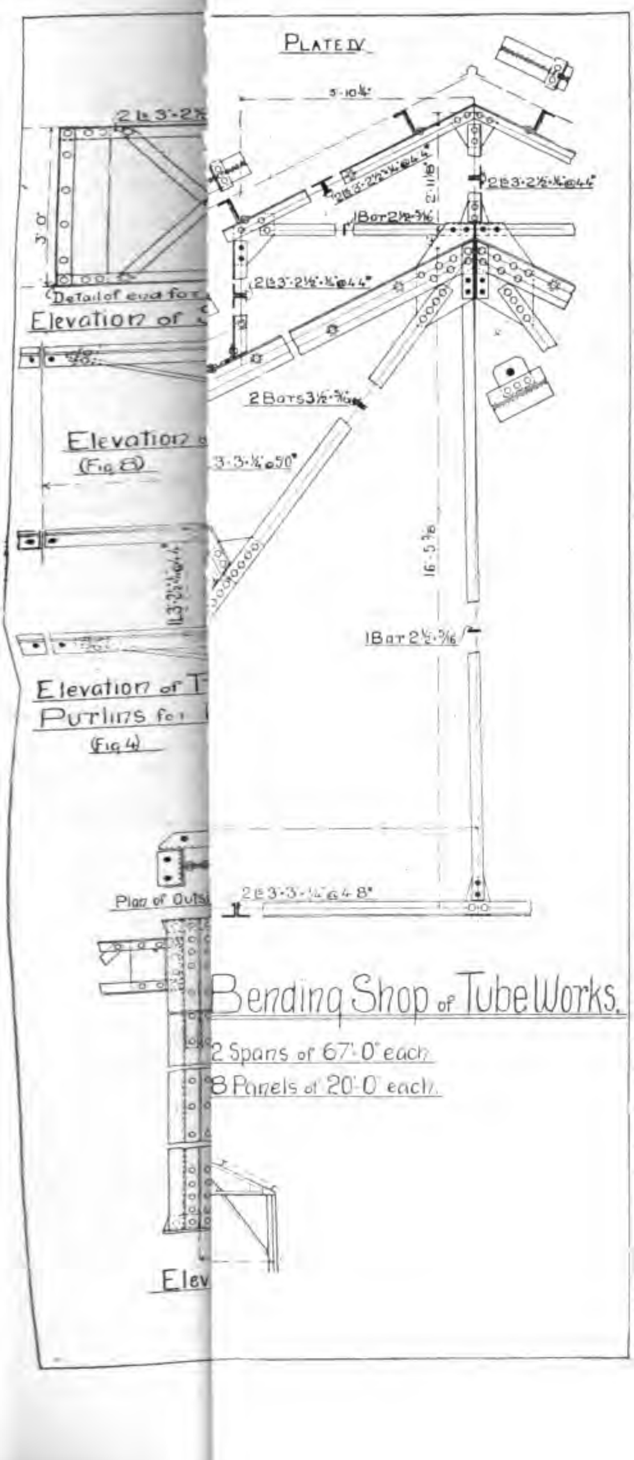


Plate III
Side view of 6×8" Post & 4×8" Cross Strip showing cut for Connection Rafter
12"
General Arrangement of All Splices
Plate 9"×5⁄8"×1'6"
Cross Strip
1⅛"
2⅛"
3"
4⅛"
Plate 9"×5⁄8"×0'11⅛" & Cut Post and Cross Strip
Side View of Main Roof Center Post
Post 6"×8"
Center Line Bottom Chord
2⅛"×6"×8" Filler Block
15" from c of Column
Point R
5⅛"×8"
7"×8"
Center Line of Col
Face timber frame work on gable end 18" out from Center Line of End Rafter
General Diagram showing position and size of Wood in end of Building for corrugated iron connection also Details for connections
Material White Pine
Keystone Bridge Co.
Pittsburgh Pa 8.12.91
C.L.W.
All holes for ⅝ Bolts
Press Shop
Armor Plate Dep't
Homestead Steel Works, C S Co Ltd
Drawing of Contract K 810
Press Shop for Armor Plate Plant
27

PLATE IV

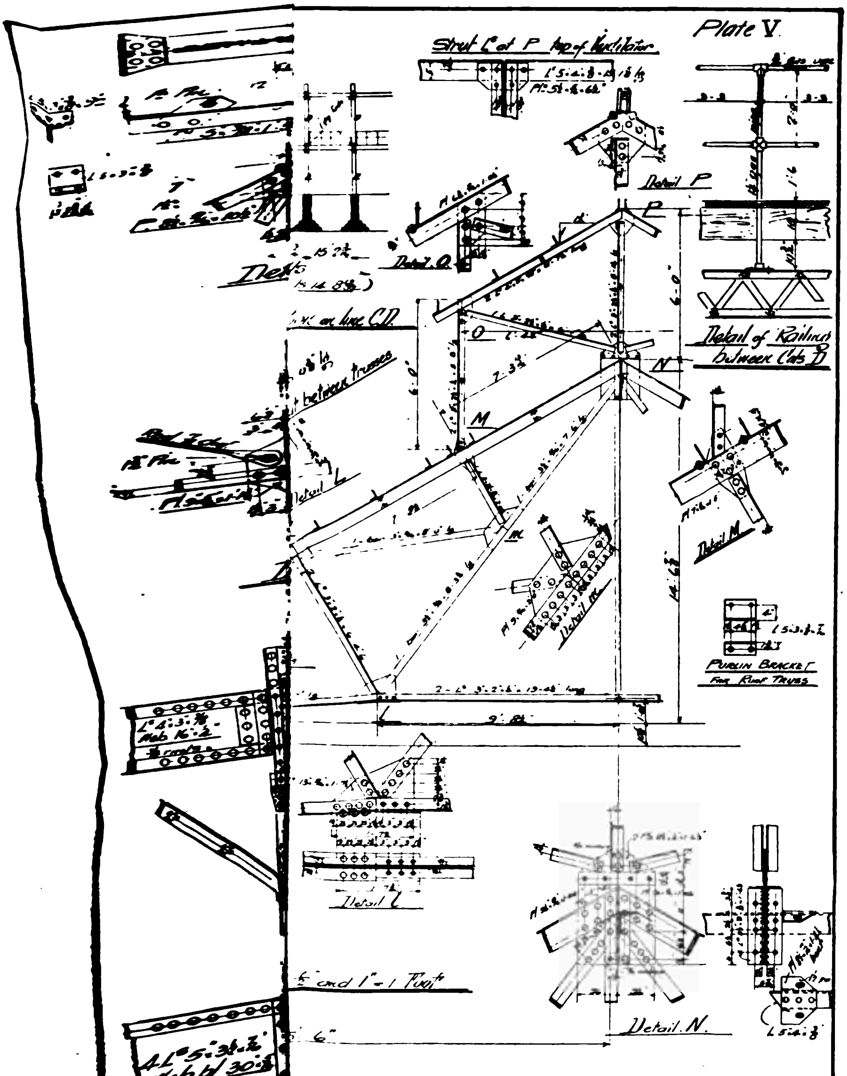


Bending Shop of Tube Works.

2 Spans of 67'-0" each.

8 Panels of 20'-0" each.

Plate V


Strut C at P top of Ventilator
Detail P
Detail O
Detail of Railing between Cols D
Detail M
Purlin Bracket for Roof Truss
Detail L
Detail N
on line C.D.
between trusses
Web pl 30
6'-0"
14'-6"
M
N
O
P
2 - L 3" 2" - 19'-4" long
9' - 8"
and 1" = 1 Foot

DETAILS - OF - COLUMNS - GIRDERS - ROOF TRUSSES - ETC

— FOR —

MACHINE - SHOPS - 500'-1" LONG x 206'-1" WIDE - BETWEEN WALLS

— ALL MATERIAL STEEL EXCEPT LATERAL RODS STRUTS RIVETS & BOLTS —

Plate VI

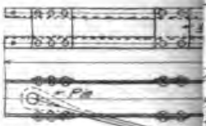
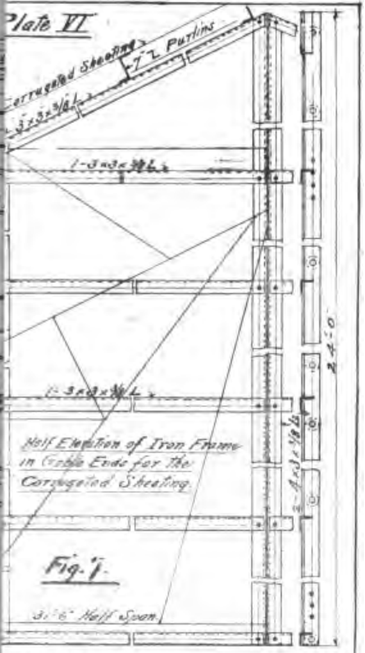


Fig. 1 Purlin

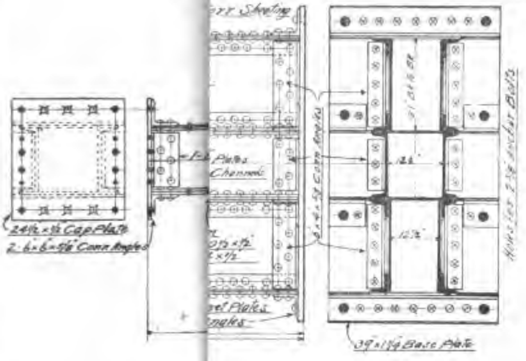
Fig. 2 Purlin

Fig. 3

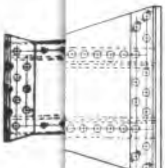


Half Elevation of Iron Frame in Gable Ends for the Corrugated Sheeting

Fig. 7.



24 1/2 × 1/2 Cap Plate
2-6×6×5/8 Conn Angles
39 × 1 3/4 Base Plate



Solving (9) for P_2, $P_2 = \dfrac{2S_2}{(2k - 3k^2 + k^3) + 2(1-k)}$, . (9a.)

$$S^1_3 = -\frac{P_2}{2}[2k - 3k^2 + k^3] + P_2 k, \quad . \quad . \quad . \quad . \quad (10).*$$

$$V_2 = \frac{m}{b} + \frac{2S^1_3(l-x) - 2P_2(a-x)}{b}, x < a, \quad . \quad . \quad (11).$$

$$V_2 = \frac{m}{b} + \frac{2S^1_3(l-a)}{b} \quad . \quad . \quad x = a \quad . \quad . \quad . \quad (11a).$$

$$V_2 = \frac{m}{b} + \frac{2S^1_3 l - 2P_2 a}{b} \quad . \quad . \quad x = o, \quad . \quad . \quad . \quad (11b).$$

Since the first term of the second member of this equation $=$ V_1, and the second $= -\dfrac{2M_2}{b}$, we may write,

$$V_2 = V_1 - 2\frac{M_2}{b}, \quad . \quad . \quad x = o, \quad . \quad . \quad . \quad . \quad (11c).$$

At point of contreflexure $2S^1_3(l-x) - 2P_2(a-x) = o$, and equation (11) reduces to $V_2 = \dfrac{m}{b}$, at point of contreflexure, or when $x = \dfrac{M_2}{S_2}$ . . (11d), or so far as the overturning action of the wind on the building is concerned, the columns have virtually been shortened by the amount $x = \dfrac{M_2}{S_2}$, which is the distance from the base of columns to the point of contreflexure. In fact an examination of the several equations for moments, shears, and vertical reactions, will show that Case II. becomes, in all respects, Case I., with the base of column moved up to the point of contreflexure.

This can also be shown by an inspection of the moment and shear diagrams on Plate I. This is true, not only for the moments and shears for the part of the columns above the point of contreflexure, but also for the stresses in trusses and knee-braces. There will occur, however, below the point of contreflexure, shear and a negative bending moment, of which the action on the column and pier must be considered.

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* In the analysis for this case the column has been considered as a beam with level supports; strictly this condition will not be realized, for as the top of the column deflects to the leeward, the support at that end will be depressed an equal amount, and the resulting stresses will be somewhat larger than found by the analysis. However, this increase is in a measure counteracted by the condition of partial fixedness at the top of the column, which has been disregarded in the analysis.

In the above formulas, several expressions have been given for the values of V_1 and V_2. For a given analysis, however, only one of these expressions need be used; the position taken for the centre of moments determining which formula shall be chosen.

For an analysis under Case I., for columns hinged at the base, formulas (1), (2), (4), and (5), only, need be used; and for an analysis under Case II., formulas (6), (8), (9*a*), (10) and (11*a*), only, are required. The supplementary formulas may be used as substitutes for those just named when so desired.

The horizontal reaction or shear at the base of the columns is a known quantity; it remains the same whether the columns are hinged or rigidly fixed at the piers. For the two columns of a bent it is equal to the horizontal component of the wind on one panel of the building and roof. This total shear or reaction is assumed to be equally distributed between the two columns. The wind-loads are considered concentrated, and the concentration at the foot of the column is disregarded in the analysis of stresses in trusses and columns, but is considered in the calculation for anchor-bolts, masonry, etc.

External Forces.—The columns, in deflecting from the wind-loads, have a leverage action producing certain horizontal reactions at the foot of the knee-braces, and at the bottom chord of roof-truss, which must be considered as external forces in finding the vertical reactions, and in the analysis for stresses in the knee-braces and roof-truss.

Vertical Reactions from Horizontal Forces.—Taking the centre of moments at any point in the axis of either column, the vertical reaction at the opposite column, in any case, is equal to the algebraic sum of the moments of all the external forces above this point, divided by the span of the building. The centre of moments may be taken at any point in the axis of the column, but the most convenient points are at the base of the column, at the point of contreflexure, and at the foot of the knee-brace.

Vertical Reactions from Vertical Forces.—The vertical reaction at either column, resulting from the vertical components of the horizontal wind-force is equal to the sum of the moments of these vertical forces about a point in the axis of the other column, divided by the span of the building.

For analyses in which only the horizontal component of the wind-force is dealt with, as in Cases 1 and 2, the vertical reactions resulting from the horizontal component only need be considered. When, however, as in Case 5, the analysis is made for the horizontal and vertical components of the resultant normal wind-pressure, the algebraic sum of the corresponding vertical reactions must be considered. In all cases, the wind- and dead-load stresses have been determined separately for the purpose of comparison, and to locate the alternate stresses. To determine the net stresses in the members and the resultant overturning action on the building, the stresses and reactions for dead- and wind-loads must be combined algebraically.

Process for Case I. For Analytical Part Only.—Find S_1 the shear at the base of the column and substitute its value in equations (1) (2) and (4); solve (1) for S_2^1 the shear at the top of the column. Solve (2) for P_1 the horizontal load at the foot of the knee-brace. Solve (4) for M_a^1 the maximum positive bending moment in the column; this bending moment occurs at the foot of the knee-brace.

Find *m* the sum of the moments of the horizontal wind-loads, taking the foot of one of the columns as the centre of moments (see Nomenclature), and divide by *b* the span of the building; this gives V_1 the vertical reaction at either column due to the horizontal component of the wind-force as shown in (5).

Process for Case II. For Analytical Part Only.—Find S_2 the shear at the base of the column and substitute its value in (9*a*) and solve for P_2 the load at the foot of the knee-brace. Substitute value of P_2 in (6) and (10) and solve (6) for M_2 the negative bending moment at the foot of the column; and solve (10) for S^1_3 the shear at the top of the column. Substitute values of M_2 and S_2 in (8) and solve for M_a^2 the maximum positive bending moment in the column; this bending moment occurs at the foot of the knee-brace. Finally find *m* the sum of the moments of the horizontal wind loads, taking the foot of either knee-brace as the centre of moments (See Nomenclature) and substitute the values of *m* and S'_3 in (11*a*) and solve for V_2 the vertical reaction at either column due to the horizontal component of the wind force. —(See paragraphs relating to *Vertical Reaction from Horizontal Forces and from Vertical Forces.*)

Process for Cases I. and II., continued. For Graphical Part Only.—The action of the horizontal forces on the columns has now been fully determined. The horizontal and vertical reactions have also been determined and the stresses in roof-trusses and knee-braces can be easily found graphically. The graphical treatment is shown in full on Plate I, Fig. 2. Taking the foot of the windward knee-brace as a convenient starting point, the force polygon is constructed as follows: from *a* to *l* to *c-d-e-f-x-y-t* to *a* to close at point of starting; the polygon being made up of the wind-loads and the horizontal and vertical reactions mentioned above. The shear at the base of the column does not directly appear in the force polygon or in the stress diagram, but its equivalent has been considered in the load at the foot of the knee-brace and in the shear at the top of the column.

Notice that at the foot of the windward knee-brace, the concentrated wind-load at that point must be deducted from the load P_1 or P_2 to find the net horizontal reaction which is to be used in the force polygon and which is the horizontal component of the stress in the windward knee-brace.

The method of analysis which has been used in Case II. for horizontal forces on buildings, is applicable to through bridges having overhead bracings and with floor beams riveted between the posts.

DISCUSSION.

MR. JOHN M. DEFORTH: I am surprised that no text-book gives an analytical or graphical solution for the stresses in knee-braces connecting the columns to the bottom chord of roof-trusses. The engineering profession should certainly feel grateful to Mr. Hutchinson for treating this case in his paper. Mr. Hutchinson assumed the full action of the horizontal wind force on the vertical projection of the roof, which seems to me too great, from the fact that only the normal component affects the stresses in the different members. Wind is not a substance that adheres to a smooth surface. It has a tendency when it strikes at an oblique angle to curve upward, thereby exerting only the intensity of the normal component. If we accept this condition as true, the stresses in columns, knee-braces, and roof-members, are functions, not only of the height of the building but also of the pitch and span of the roof.

Using Unwin's formula for the pressure normal to surface of roof, $A = F \mathit{sin.}\, a^{1.84 \cos. a - 1}$, in which $F =$ force of wind in pounds per square foot, and $a =$ angle of roof with direction of wind, I find the stresses in the knee-braces on Sheet I. for Case 1, respectively 55,500 pounds and 81,000 pounds.

In graphical solutions it is best to combine the wind-loads with the dead-load, as the intersection of the resultant of these loads with the base-line gives a true criterion for the stability or instability of the structure. If the resultant intersects the base-line between the columns, the structure is secure against overturning, otherwise there is an upward pull on the windward column, which must be resisted by the anchorage. Flat and wide roofs are more stable than steep and narrow ones of the same height; and hip-roofs for small isolated buildings are preferable to vertical gable ends. Closed and high ventilators on light and narrow buildings have a great effect on the overturning of the structure, therefore, light buildings, especially, should be designed with the knowledge of the stresses resulting from wind.

MR. CHAS. H. NICHOLS: I think Mr. Hutchinson has given us some very novel and valuable information in his paper this evening. I am very much pleased with his treatment of the wind-force and its action upon the columns, knee-braces and trusses. This is a question which has attracted my attention for some time, and it was only a few weeks ago that I made a successful solution of the problem. I only considered at that time a slight modification of his first case, and I wish to say that the results he has obtained correspond very well with those I found. This is very gratifying to me, indeed. So far as my limited knowledge extends, I believe the authors of text-books on this subject have overlooked, or have shamefully disregarded these very important strains. It is perhaps in a measure due to this fact that engineers have also disregarded them.

Two years ago I was engaged in the design of a train-shed for the Providence terminal, and it was there that I first got entangled in this little snarl which Mr. Hutchinson has unravelled for us this evening. It was desired by the Chief Engineer to have a shed constructed with the trusses resting on columns 30 feet high, and to resist a horizontal wind-force of 30 pounds per square foot. We got such enormous bending moments in the columns that we finally resorted to a three-hinged arch design, which was found to be far more satisfactory. It cannot be denied that these enormous strains do exist even under the action of a moderate wind force, and, if it is essential and consistent to compute the strains which we ordinarily consider in roof-trusses, it appears to me just as essential and consistent to compute these wind-strains. It would be a little more scientific designing than the old "rule-of-thumb-way" of guessing at it.

In regard to the question of wind, I wish to state that in the construction of the P. R. R. train-shed at Jersey City the wind-load was applied horizontally and was taken at 35 pounds per square foot.

There is one interesting feature in connection with the knee-brace problem which many of you no doubt have observed. When the columns are hinged at the base, as in Case I., the strain in any knee-brace, which is tangent to a given circle, whose centre is at the point A, Plate I., Fig. 5, is constant. The inclination of the knee-brace may be changed, but so long as it is kept tangent to the given circle its strain remains the same. This can be shown as follows: Taking moments at A, the knee-brace strain $= \frac{Sl}{R}$. In this expression all the terms are constant. This can also be shown by the resolution of forces. The horizontal component of the knee-brace strain $= \frac{Sl}{l-a}$ which, multiplied by the secant of θ, = knee-brace strain $= \frac{Sl}{l-a} \sec. \theta$.

The secant of $\theta = \frac{l-a}{R}$ which substituted in the last expression gives knee-brace strain $= \frac{Sl}{l-a} \times \frac{l-a}{R} = \frac{Sl}{R}$ the same as by moments.

Mr. H. J. Lewis: I have noticed that whenever people dispute about a thing, it doesn't make much difference whether it is science or religion or what it is, it is a very good sign that they don't know all about it. That is my opinion of this wind question. I do not know and I do not believe that any of us know exactly the influence of wind on a building, and I am inclined to take one honest assumption which approximates the facts as quickly as another. I do not know whether the wind acts entirely in a direction normal to the surface of the roof, or whether it can be considered as acting in a horizontal direction against this surface. I am inclined to think that either is a fair hypothesis, and that they will give very nearly the same results if the same horizontal wind-force is used in both cases.

But in the analogy that Mr. Hutchinson carries out between the railroad bridge and a building, I think that the importance of considering the wind-load on a building is considerably greater than it is in a railroad bridge of ordinary span. The wind-strains produced in the main members of a building which is high, and narrow in comparison to its height, are considerably greater in proportion than the effect of wind on a railroad bridge, unless it be a very long one. In short spans they really amount to a very small percentage.

There is another thing which I wish he had emphasized even more. (Mr. Lewis then read a short paragraph from the paper beginning "After the material has been provided to carry a given load, etc.," and then said) I have noticed in several cases in the estimation of various classes of work, including highway bridges, railway bridges and roofs, that after you have, as he says, enough material to make the structure stand, the ratio of increase in strength is considerably greater than the increase in cost. Take, for instance, a highway bridge calculated for 80 pounds per square foot and another for 100 pounds, there is an increase of 25 per cent. in actual carrying capacity, but the increase in cost will very seldom run over 12 to 15 per cent. The average increase in the strength is very nearly twice as much as the increase in cost. I have found the same principle to hold roughly with relation to railway bridges and buildings.

THE PRESIDENT: In the case of a building, where would you put that increase of strength; that is, the larger portion of it?

MR. LEWIS: I would distribute it over the building.

THE PRESIDENT: Merely distribute it, and thereby decrease the unit strains all around?

MR. LEWIS: The increase in strength comes principally from the use of larger sections in the compression members. Permissible unit strains become larger as you increase the size of the members. For instance, 25 per cent. of increase in capacity may allow you to use a 12 instead of a 10-inch channel, and you get the advantage of a larger radius of gyration. These things all go to make the increase in capacity and strength greater than the increase in cost. For a given number of pounds increased weight you get more than the relative proportion of increased strength.

MR. GEO. S. DAVISON: Like Mr. Lewis, I feel that when the doctors disagree on such a very important matter as how the wind acts upon a roof, I am willing to accept any reasonable theory. The author's plan of assuming the effect of the wind to be as though it were acting against the vertical projection of the roof and ventilators is on the side of safety, so far as the assumption respecting horizontal forces and their effects are concerned, as for a given case, the result for the shear at the foot of the columns would be a maximum, thus increasing the stresses in the knee-braces and rafters over those obtained under other assumptions. There would be a slight diminution in the vertical component of the wind-forces from the case in which the wind is assumed to carom on the roof and exert its energies in a direction normal to the same. This would give smaller results for the calculated stresses in some few of the truss members, but as there is necessarily considerable guess-work in all the forces acting on the building outside of its own weight, which latter may be, but seldom is calculated, I think there can be no room for a quarrel on the question of how the wind acts upon the roof. It may be wise, however, in designing buildings, to consider both theories so far as to make sure that those members which may be subject to alternating stresses are identified. It would have been interesting if the author had had the time and patience to have caused the wind to blow both ways upon this particular building. However, he already deserves our thanks for what he has done in his very admirable paper.

Mr. G. Kaufmann: I think in a paper of this kind which is so complete and useful, it might be wise to include also in it the unit stresses required. I think that the stresses for mill buildings should be different from those used in bridge trusses. The unit stresses used for wind should be high, in my opinion, while those for concentrated loads should be rather low. Of course, for the dead load they should be high. It might be wise to put them in the paper.

Mr. Emil Swensson: As to the unit stress for wind loads being higher than for others, I do not think that is necessary. When you assume small wind-loads it amounts to the same thing. Mr. Hutchinson assumed only twenty pounds. To assume loads is one thing and unit stresses another. What is the sense of assuming heavy loads and then to virtually reduce these loads by using higher unit strains?

Speaking about wind action on buildings, I will only state that I agree with what Mr. Lewis said about different theories and in addition will tell a few things about buildings that have actually been put up here. The late Captain Jones had certain well-defined ideas about how he wanted a mill building constructed, which goes to show that he appreciated the effect of wind-strains. First he wanted very long anchor-bolts. In the new rail mill I think they are fifteen feet long and rather large in diameter.

Another notion he had was that instead of using the ordinary knee-brace, he wanted a solid web from the post up through the bottom chord to the top chord of truss, filling in the whole of the first panel in the truss. This goes to illustrate that he was well aware of the necessity of providing for the wind strains at the vital points.

ıother point I wish to illustrate is, we should be very liberal signing mill buildings because we do not know to what use will be put, especially in large plants. I know of one buildvhich was designed to stand the strains of 20-ton jib cranes. building stands to-day the strains of 50-ton jib cranes and vhole building sways perceptibly from one end to the other, h, however, is not in the least dangerous, as long as this sway- within certain limits.

know of another case where we put up several buildings for a in purpose. They were very high, narrow and short, but were designed amply strong for their purpose. But it was ıore than two months after they were put up until a jib crane ıpplied to the outside of one of the corner columns in each ling.

another case where one column inside of the building was ut and a girder introduced to support the truss resting on column, the opening under the girder was later on filled in a brick wall, however, leaving a small opening for passage railroad rail sideways out of building, which of course neated the hanging up of said wall to the girder, although the r was not originally supposed to carry this wall.

ıere is one common misunderstanding I wish to call attention hich arises when an architect and a bridge builder, or an ıeer are working on the same building; a building in which rchitect puts up the walls and the bridge builder puts on the The architect expects the walls to be held in line by the and consequently makes a thin wall without any pilasters. engineer puts up the roof and expects to brace the roof with valls. That is a case of "the blind leading the blind." I of one case where a strong wind came up and blew in one and the other out. The architect and the owner came to onclusion they would sue the bridge builder for not bracing vall by the roof.

fact the roof should be made stiff enough in itself to stand ind-strains, and the walls at the same time should have suft thickness and enough pilasters distributed in them to make stand up by themselves and carry all wind-strains to the ıd.

Mr. W. L. Scaife: Mr. Hutchinson has shown how important the roof-strains, as produced by wind-pressure are, and it therefore behooves us to find out whether the method of calculating these strains is correct. Prof. S. P. Langley, as we all know, has made a number of valuable experiments on the influence of wind on planes at different angles. He has found that some of the formulæ ordinarily accepted, are not correct. There is an analogy of course, between the action of the wind on a fixed roof and the action on a moving plane at an angle to its direction. Therefore I think you gentlemen who are particularly interested in the action of wind-pressure on roofs should look at Prof. Langley's results, and I think you will find that the present method is not entirely accurate, when applied to the sloping sides of roofs.

Mr. Albree: The pressures vary with the velocity of the wind.

A Member: Mr. Hutchinson spoke of securing the end of the truss to the column with bolts and rivets. Would that make the column fixed at that end?

Mr. Hutchinson: Theoretically it would, but probably we would not fully realize that condition.

A Member: Do you know whether fixing the column at the top would reduce the wind-stresses?

Mr. Hutchinson: It would reduce them in the roof-trusses, knee-braces and columns.

Mr. Kauffman: Referring to Prof. Langley's experiments as to the pressure of wind; in these the wind-pressure was applied to the under side of the surface. He found that the greater the velocity the greater the lifting force of the wind. In the case of wind applied to roofs it acts on the upper surface, consequently the reverse of his calculations would be true, and there would be a greater pressure than is usually provided for. I think it would be a good idea to look that matter up carefully.

Prof. Mansfield Merriman: Mr. Hutchinson's analysis of columns under the action of horizontal wind-pressure, seems very valuable, and I regret that the short time allowed for discussions prevents me from making a detailed study of it. The second case, where the column is rigidly fixed at the base, is likely to be the important one in tall-building construction, and the equations developed are simple and capable of direct application. The assumption of horizontal wind-pressure on the roof is slightly at variance with ordinary practice, but it is probable that it gives a sufficiently near representation of the stresses caused by wind. Indeed, it seems to me as if the most dangerous effect of the wind would be an uplifting one, or, with varying gusts, perhaps, a twisting one also. The whole investigation of wind-action given by Mr. Hutchinson is an excellent one, which will tend to secure stability and economy in iron-building construction.

MR. E. K. SCOTT: Mr. Hutchinson has gone over the subject of mill buildings very carefully and thoroughly. One point not mentioned in his paper may, however, be added. When channel sections are used in the construction of the rafters or top chords for the type of trusses shown on Plate II. for spans of, say, 70 feet or under, the actual sections of the rafters are generally so much in excess of the theoretical requirements, for the sake of good detailing, rigidity, etc., that the sub-posts, Bb and Dd, at the quarter points, as well as the horizontal tie-rod, Cd, between the foot of the upper sub-post and the top of the middle post, may be omitted in most cases without impairing the strength of the structure to any perceptible degree. If need be, a stiff member may replace the usual tension-rods extending from the foot of the middle post to the peak, c to E. This would decrease the number of pieces in the truss and the amount of shop-work depending thereon. Both of these items are important in estimating the cost of the structure.

MR. HUTCHINSON: Referring to the question of normal *vs.* horizontal pressure of the wind on roofs, which seems to have received its share of attention in this discussion, I would say that, on account of the shortness of time, it has been impossible to treat the number of cases for wind which had been contemplated, but, for the revised *Transactions*, as has been suggested, analyses will be made for additional cases, covering normal wind-pressure. I will also introduce in the text Unwin's formula for normal wind-pressure, which has been suggested by Mr. Deforth, and will supplement with a table of values for the normal pressure.

There is one point which I believe has not been mentioned. I should judge that the presence of a ventilator would materially affect the action of the wind on account of the retarding effect which the vertical side of the ventilator offers to the current as it leaves the main roof-surface. I would like to inquire whether any one present has data respecting this point?

Mr. Davison: I think that the wind, if simply impinging on the roof, would affect the pressure on the side of the ventilator.

Mr. Hutchinson: It would seem reasonable that the retarding effect would also influence the wind-pressure on the main roof-surface, although in what manner experiment alone can determine.

Mr. Kaufmann has suggested that a statement of the unit stresses for mill-buildings be added to the paper. I would say that I have intentionally omitted reference to either unit stresses or material. These two questions are necessarily related, and would require, for a proper treatment, an entire paper in themselves. I, for one, would be pleased to see such a paper written up.

Mr. Scott's suggestions respecting the omission of secondary web members and the substitution of a stiff member for the tie running to the ridge of the roof truss are excellent and worthy of careful consideration.

I would like to add one or two examples to Mr. Swensson's list, illustrating the uncertainty respecting the loading which is liable to occur in a mill-building of the present day. I call to mind one building, a machine shop, in which the travelling crane was increased from thirty to sixty tons. In another shop, it is contemplated substituting a one-hundred-and-fifty-ton crane for two sixty-ton cranes acting together. Fortunately, the buildings were designed far in excess of their original requirements.

Adjourned.

R. N. Clark,
Secretary.

October 25th, 1892.

Minutes of Chemical Section Meeting.

Nineteen members were present.

Mr. R. N. Clark was chosen temporary chairman. The minutes of the September meeting were read and approved.

Professor Francis C. Phillips was nominated for Chairman of the Section, and, as there were no other nominations, the secretary was instructed to cast a ballot for Professor Phillips.

Professor Phillips, on taking the chair, thanked the members for the honor done him in making him the successor of so able a man as Professor John W. Langley.

Mr. Joseph Eastwick read a paper on "The World's Supply of Nickel," in which he described the sources of nickel-ore supply, and showed specimens of the ores. The discussion brought out much interesting information.

Mr. James O. Handy read a communication giving the results of sulphur determination in coal by Eschka's method, compared with Hundeshagen's modified method.

THE WORLD'S SUPPLY OF NICKEL.

BY JOSEPH H. EASTWICK.

Nickeliferous deposits are widely distributed over the surface of the globe, and in many countries the ores have been worked to a greater or less extent; Saxony, Norway, Sweden, Spain, Russia, the United States, have all added their quota to the world's supply of nickel. The percentage of nickel contained in the ores that have been considered worth working, have varied from 2 to 10 per cent. The ores of the present day are sulphides, arsenides, and silicates. We refer back to the year 1751 to find nickel first isolated by Cronstedt. It was first noticed as a peculiar metal in the copper-ores of Germany, and was called by the miners, kupfer-nickel, or false-copper, or the old-nick in the copper, on account of the trouble it caused them in refining the copper; it was then regarded as a useless and injurious substance. Within the last fifty years nickel has found various applications in the arts, particularly in the manufacture of German silver, this being an alloy of copper, zinc, and nickel, and in nickel-plating, and more recently as an alloy with steel. It has also been used, to a limited extent, as an alloy with copper for coins; in this country the five-cent piece being an alloy containing 75 per cent. copper and 25 per cent. of nickel. In Germany they have a coin of pure nickel. Nickel is always present in meteoric iron.

At the present day, the world's supply of nickel may be said to be derived from two extensive deposits—the one in New Caledonia, an island in the Pacific Ocean belonging to France, and the other at Sudbury in the province of Ontario, Canada. There are several deposits in the United States which are believed to be extensive and valuable, but which have not yet been explored—namely, in Oregon, Nevada, and North Carolina. We have some specimens here of ores from these localities, and it may be interesting and instructive to you to become familiar with their appearance.

The character of ores most used at the present time are the nickeliferous pyrrhotites or magnetic pyrites, and the silicates of magnesia and nickel, or so-called Garnierite or Genthite. One of the oldest nickel mines in the United States, where regular mining has been carried on, is in Lancaster County, Penna., known as the Gap mine. This property was bought a number of years ago by Mr. Joseph Wharton, and mining has been carried on there with intervals of interruption ever since. This deposit consists of a nickeliferous pyrrhotite and chalcopyrite, a sulphide of copper and iron, in which the nickel replaces a portion of the iron. The average percentage of nickel in this ore is between two and three per cent. The ores from this mine have all been used at the American Nickel Works, Camden, N. J. Mr. Wharton was the pioneer in the nickel industry in this country, and, until within the last two years, was the only manufacturer of nickel in the United States. Within the last few years the mines in Lancaster County have shown signs of exhaustion, and Mr. Wharton has been obliged to go elsewhere for a part of his supply.

Probably the most extensive and important deposit of nickeliferous pyrrhotite to be found anywhere is the one at Sudbury, Province of Ontario, Canada. Mining is now carried on here on a large scale, and the product sent to the United States and

Europe in the shape of matte. The average percentage of nickel contained in these ores is between three and four cent. As a rule, the average percentage of copper and nickel contained is about the same, but in a number of specimens that I have examined I have found that the higher the percentage of copper contained the lower the nickel, and *vice versa.* The ores from the Sudbury district are remarkably free from arsenic, and contain about $\frac{1}{8}$ per cent. of cobalt. The deposit consists of massive pyrrhotite (Fe_7S_8), intermixed with chalcopyrite ($CuFeS_2$). The celebrated arsenide of platinum, Speirylite, occurs in the district, and is found in a brown earth. The future of the Sudbury mines promises to be very important, as new developments are being constantly made, making an increase in the output of ore.

A remarkable deposit of nickel ore was discovered about twenty-five years ago in New Caledonia, by Garnier, who was then attached to an exploring expedition in that island under the auspices of the French government. It was named after him Garnierite. It is a double silicate of magnesia and nickel, and is found in veins traversing the serpentine rocks near Noumea, the capital of New Caledonia. It is associated with chromic iron and steatite or Talc (silicate of magnesia). It is of a soft friable nature of Sp. Gr. 2.3 to 2.8, and of a pale-green color. The average percentage of nickel contained is between 8 and 10 per cent. There is no cobalt present in the ores, and in that respect, as in many others, they differ from most nickeliferous ores. Mining has been carried on here on a large scale, and the New Caledonian ore supplies most of the world's nickel, but the enormous developments in the Sudbury district in Canada promise a large increase in production in that quarter.

The following are some analyses of specimens of New Caledonia ore by Readman :

Oxide of nickel,	11.08	12.68	18.04	24.28
Oxide of iron,	7.34	7.14	.88	3.80
Alumina,	1.56	1.50	1.24	
Lime,	1.90	.72		.58
Magnesia,	22.50	21.83	16,37	15.35
Silica,	37.96	43.41	45.74	40.24
Water,	17.30	12.38	17.28	16.21
	99.64	**99.66**	**99.55**	**100.46**

A nickel ore has recently been discovered at Webster, Jackson County, North Carolina. It is similar in many respects to the Garnierite of New Caledonia, being a silicate of magnesia and nickel. It is amorphous, of a yellowish-green to emerald-green color; it is soft and friable. No regular mining has been carried on, nor has any great effort been made to ascertain the extent of this deposit.

A nickeliferous ore has been found at Riddle, Douglass County, Southern Oregon, resembling in many respects the nickel ores of New Caledonia. This deposit is believed to be extensive. The color of the ore is light-green. No regular mining has been carried on here.

Another ore, an arsenide of nickel (kupfer nickel or nickelite) has been found at Churchill in Nevada. It is of a dark-green color. Several car-loads of this ore were sent to England, but none has been used in the United States.

A slight reference to the modes of working the New Caledonia and Sudbury ores might not be out of place here.

The New Caledonia ores are either reduced to a matte containing from 50 to 60 per cent. of nickel and then shipped to European works, or are shipped as ores containing from 8 to 10 per cent. of nickel. The subsequent operation is entirely a dry metallurgical process, and no solution of the ore is made to extract the nickel. The matte to be refined is mixed with gypsum and smelted in a cupola or reverberatory furnace, the gypsum and metals being converted into sulphides, or the ores may be treated directly with gypsum and smelted in a furnace. The regulus thus obtained is then refined, being ground and carefully roasted in a reverberatory furnace in order to convert the iron into oxide, the nickel remaining as sulphide. On removing the charge from the furnace it is mixed with sharp sand and the mixture treated in a reverberatory furnace. The oxide of iron combines with the silica of the sand, the nickel still remaining as sulphide. The iron slag is drawn off at one opening and the nickel sulphide at another, and the latter is run into moulds. This process is repeated until the iron is completely removed or nearly so. The sulphide of nickel thus freed from iron is ground and exposed to a dull red heat on the hearth of a reverberatory furnace, and is converted into oxide. The oxide is reduced by charcoal to metal containing between 98 and 99 per cent. of nickel.

The nickeliferous ore of Sudbury, averaging between 3 and 4 per cent. of Cu and Ni, is roasted and then smelted in a cupola furnace to a matte, containing about 20 per cent. each of copper and nickel. This matte is shipped to Cleveland where the Canadian Copper Company's works are located, and to the Orford Company's works near New York, and to other nickel works. After enriching the matte to about 40 per cent. each of copper and nickel, the metals are dissolved out with acid. The Orford Company's process, however, is said to be an entirely dry one, but the details are kept a secret; but in a general way it may be said that they work on a large scale as refiners of copper ores, and the nickel oxide is obtained as a by-product. The oxide is not reduced to metal by them, but is sold as oxide for alloying with steel and to the nickel manufacturers, who reduce it to metal.

We have here a number of specimens of nickeliferous ores for which we are indebted to Mr. Joseph Wharton, of Philadelphia. and Mr. Voight, of the American Nickel Works; to Dr. Emmens, President of the Emmens Metal Company; to Mr. H. P, McIntosh, of the Canadian Copper Company, and Mr. S. A. Ford of the Edgar Thomson Steel Works.

For the purpose of indicating the relations of all the known nickel-iron sulphides to each other, the following table may be found useful:

	Ni.	Fe.	S.
Millerite, ...	64.72	......	35.28
Polydymite, ...	59.47	......	40.33
Beyrichite, ...	57.90	......	42.10
Polydymite, ...	44.92	14.26	40.82
Folgerite, ...	32.87	31.30	35.83
Pentlandite, ...	22.03	41.95	36.02
Horbachite, ...	11.24	42.81	45.95
Inverarite, ...	10.44	49.72	39.84
Whartenite, ...	6.10	40 68	53.22
Blueite, ...	3.76	42.96	53.28

DISCUSSION.

The CHAIRMAN: I think we all regret that Mr. Eastwick had not made his paper longer, so as to enter more into detail, and give more particulars about some of the matters discussed; but he is probably ready to talk at length if we call upon him.

MR. EASTWICK: Mr. Mixer is here, and he is probably familiar with the subject of nickel ores.

The CHAIRMAN: Mr. Mixer, can you enlighten us any further on the points brought up in this paper?

MR. CHAS. T. MIXER: I do not know that I can add much to Mr. Eastwick's paper; he has covered the general ground. However, there are two or three things that I will speak of which may be of interest. Mr. Eastwick spoke of the Sudbury mines, and I will say that I visited these mines this spring, in connection with my work for our company; I looked over the grounds, and saw just how they treat the ore. The ore that is mined there is pyrrhotite, as Mr. Eastwick says, and contains from 2 per cent. to 4 per cent. of nickel. The ore is made into large heaps, is set on fire with cordwood and roasted for 60 or 70 days. At the end of this time, the greater portion of the sulphur is gotten rid of. The sulphur is given off in dense clouds, and spoils all the vegetation around. These roasted heaps are then broken down, and smelted with coke in cupola-furnaces, and, as Mr. Eastwick has said, it is thus purified in a degree; the nickel is brought up to about 20 per cent., copper about 20 per cent., iron about 30 per cent., and the balance sulphur. This is shipped as matte, and then comes the work for such companies as the Orford Copper Company, to turn this into nickel oxide, which they do. They have a secret process for doing this; nobody knows exactly how they do it, but they manage to turn it into almost commercially pure nickel oxide, which is sold to different companies, either for running down or adding to nickel-steel. There are a good many ways in which they may do this. If they will take the matte, partially roast it, put in sand, and slag off the iron (as Mr. Eastwick says, the sulphur tends to cling to the nickel in preference to iron, and the iron becomes oxidized and slags off), they can purify it to a certain degree; but how they get it up to so high a percentage of nickel, with but a small amount of copper and iron, as they do, is kept a secret by them, and people can only guess how they do it, in the dry way.

As to the North Carolina ores, we have had a little experience with them, at the suggestion of Mr. Ford, of the Edgar Thomson Steel Works, and, I am sorry to say, they have not been satisfactory at all. We spent a little money down there; in fact, got some shipments of ore at our place; and when we came to mix it all up it averaged only about 2 per cent. of nickel, and, of course, it was not worth paying any attention to. There are some samples of this ore on the table here to-night, running up to 10 per cent.

The United States cannot be said, at present, to produce nickel in commercial quantities. The Gap mine, in Pennsylvania, for many years produced considerable quantities, but is now idle.

The Nevada mines are rich, but they have two disadvantages; one is, that they are so far away from railroad facilities, and the other is, that the ores are, unfortunately, arsenides of nickel, which are difficult to refine; and, consequently, nothing worth mentioning has been done with the Nevada ores, although there in quantity and comparatively rich.

The Oregon ores are of the silicate class, and, as yet, do not seem to have been developed in commercial quantities.

There is a new mine in Colorado, the Gem mine, to which our attention has been called. I believe the ore is an arsenide, but nothing much has been done with it as yet, although it promises very highly.

This leaves New Caledonia and Canada the two great nickel-producing countries at present. The Canadian Copper Company will now probably run the New Caledonia people a pretty fast race, as they have an extensive plant there now, having put about $2,500,000 into it, and they have several mines doing very well.

Among the samples here, I would say that there is a piece of matte which is of rather higher grade than I have just spoken of, namely, 20 per cent. This sample runs to about 60 per cent. It has been refined two or three times, so that it has only a small percentage of iron in it; and I want to call your attention to the similarity of that and a little piece of white sulphide of iron which

I picked up in a well near our works. It has no nickel in it at all. Put the two together, and probably one of the Canadian Copper Company people would say that both were pieces of their Bessemer matte.

There is another point I will speak of. There is a mineral in Canada that the miners there call Millerite. I notice a sample here in Mr. Eastwick's collection. Mr. Eastwick speaks of it as Millerite, and it is a common thing to do so. Dr. Emmens set me to work a little while ago to examine this mineral, and we found that it was not true Millerite at all; the little plates could be separated from the pyrrhotite, being non-magnetic. When these were analyzed, we found the composition to be NiS,FeS—sulphide of iron and nickel in equal molecular proportions—and Dr. Emmens, after looking up the subject thoroughly, named it Folgerite, after Commodore Folger, of the U. S. Navy. I think if any of you ever have the time to look into the matter, you will probably come to the same conclusion, namely, that it is not true Millerite. I have a piece here of true Millerite. You will notice the little acicular hairs in the cavity, which is the true form of Millerite.

One of the most curious combinations that we have found in Canada, was a combination of niccolite and gersdorffite—the arsenide and the sulph-arsenide of nickel. The specimen, as you may judge from the weight, must have a good deal of metal in it. The arsenide, of course, is very high in nickel; the sulph-arsenide, the gersdorffite, is considerably lower; but if a mine of that sort of article could be found now, producing it in any quantity, it would be a very rich find.

The CHAIRMAN: I would like to ask you whether there is not a process, said to be patented, for the extraction of nickel from its ores, based on the formation of a volatile compound of nickel and carbonic oxide; have you heard whether the process has been successful?

MR. MIXER: I have heard of the process, but never heard of any quantity of nickel being turned out by it.

The CHAIRMAN: It was said to have been patented.

MR. MIXER: Yes, sir. I know the process you refer to, but have not heard of its being successful at all. Mr. Wharton, who is really at the head of the nickel industry in America, as Mr. Eastwick said, and who, up to two years ago, was the only refiner of nickel in America, has his own process, and he has always done it in the wet way, and reduced from hydrates, etc., but it is a very expensive method. The dry methods are considered the best. The Orford Copper Company have a dry method, which they keep secret. Speaking of wet methods, I have one little curiosity here which I think would puzzle almost any chemist or mineralogist if he saw it in a collection of minerals. In our refining department we use lead-lined kettles, and dissolve the nickel in them for making salts. One day, curiously enough, the solution became concentrated, or in some way got baked in the bottom of the kettle, and some basic sulphate of nickel was formed. Here is a sample of the product; it looks very much like natural nickel-ore, but really it is an artificial product from the kettle.

Here is a sample that I ran across in Canada, at a mine called the Sheppard mine. You will notice how sparkling the crystallization is. On trying it in a closed tube, I got sublimate of sulphur, which placed it as a *di*sulphide of iron. After analyzing it more carefully, I found it was disulphide of iron, in which $Fe:Ni = 7:1$; that is, the formula would be, $NiS_2,7FeS_2$. It was something new. We made an analysis of it, and Dr. Emmens called it Whartonite, after this same Joseph Wharton, of Camden, N. J., who is the oldest nickel refiner in this country. It is a disulphide of iron, which contains nickel. It is a very unusual thing for pyrite to contain nickel in such large quantity. Of course, pyrrhotite is virtually a monosulphide of iron, and that will contain nickel up to several per cent., but disulphide of iron containing such a quantity of nickel was unusual. No text-books that we ran across gave an instance of so proportionately high nickel percentage in a pyrite.

The CHAIRMAN: It is said, I think, that Norwegian ores were used very largely in the manufacture of nickel for the German coinage a good many years ago. I think that ore was said to be a pyrite. Can any one say anything about that occurrence of the ore?

Mr. Mixer: No, sir, I could not; except that if it was a pyrite, as I was just saying, it must have been very low in nickel. I think, however, it was a pyrrhotite, and contained a very small percentage, and, of course, that makes refining a very expensive business. When it is found in such small percentage, there is so much impurity to get rid of. But I do not know how they handled the article at all. I suppose they roasted it in the usual way, and concentrated it by slagging off the iron and keeping the nickel; but I do not know the facts in the case.

The **Chairman**: It may have been extracted by some wet method?

Mr. Mixer: That may have been.

The **Chairman**: Mr. Eastwick, can you say anything about that occurrence in Norway? It was said to be a very important occurrence a good many years ago. There was a specimen of the ore at the Centennial.

Mr. Eastwick: I have not looked into that matter.

The **Chairman**: There is a statement made by nickel-platers very often that there is a peeling off of the nickel plate when there is a little arsenic in the nickel. Is that a fact, do you think?

Mr. Mixer: Yes, sir; the nickel-platers, I believe, are very much opposed to having a trace of arsenic in their nickel salts. It has the effect stated. They are also very much opposed to having a trace of copper, which immediately turns the deposit black. A little iron getting into the deposit does not seem to hurt it much; in fact, if anything, brightens the color. But if any large proportion of iron gets in, it has the effect stated; so that, in general, nickel-platers desire practically chemically pure products in the form of nickel salts, etc. They are especially opposed to copper and arsenic.

Mr. Eastwick: Mr. Mixer, I understand that there is a method for welding nickel and iron; that is, rolling the two metals together so as to form a homogeneous sheet. Do you know whether that has been carried out to any extent, and whether practicable?

Mr. Mixer: Yes, sir; I am very sure it is, for we received sheets from a company in Cleveland, O., as we had occasion to use some. Looking at the edges of the sheet, it cannot be noticed where the nickel ends and the steel begins; no joining at all can be noticed; but, of course, the outside is mostly nickel, and the inside, steel. They take a sheet of steel and then weld the nickel to both sides of it. It makes, of course, a non-oxidizable article, having the stiffness of ordinary steel, which would be very handy for various uses. I can say with safety that they are manufacturing this and making a success of it.

The CHAIRMAN: Mr. Eastwick, I think you did not mention in your paper whether nickel ores can be obtained from the Urals?

MR. EASTWICK: No, sir; I do not think the ore is obtained to any extent there.

The CHAIRMAN: I remember seeing a specimen of what was said to be almost pure oxide of nickel at the Centennial that was said to have come from somewhere in the Urals.

MR. EASTWICK: I have not found anything in regard to the Ural deposit, and do not remember about any regular mining going on there as in New Caledonia or in Canada in the Sudbury district. I do not find any mention of their being large producers.

MR. MIXER: Speaking of nickel oxide reminds me of a report coming from Oregon recently that metallic nickel was found native there.

The CHAIRMAN: I believe a deposit of metallic zinc has been found in a locality in Tennessee, and I suppose that if zinc is found native we may look for almost anything. I think these facts which Messrs. Eastwick and Mixer have brought out illustrate one interesting point in regard to the rarer metals, and that is, that as soon as what has been considered a rare metal finds many uses, new localities appear everywhere. *Nickel-steel* is of special interest just now, and it may be that some of the members can tell us something about its application to armor-plate.

MR. MIXER: I know very little about armor-plate, but I believe that in the armor-plate they intend to use a very small percentage of nickel; my recollection is that it is somewhere around 3 or 4 per cent. I think the general impression abroad is that the nickel-steel would contain possibly a quarter or a half per cent. of nickel, and people have often said to us: "That must increase the nickel business a good deal?" But when you come to think of the small percentage of nickel in the steel, it will be seen, of course, that it will not increase the demand for nickel so much as is generally thought. The other day, Commodore Folger sent us a bar of nickel-steel that he had had made containing 25 per cent. of nickel. It was very tough. It was about 2 inches in diameter, and a steel chisel was ruined trying to cut through it. We had it buffed, and it took a beautiful polish. For such things, therefore, as pistol-barrels, I think steel with high percentage of nickel may prove very valuable; for, of course, when the percentage of nickel rises, the chance of its oxidizing is very much lessened.

Mr. Eastwick: I would like to ask the gentleman whether he has heard anything about the Oregon deposit, and whether any developments have been made.

Mr. Mixer: I have heard nothing of it, except, as I said, there is very little being done there.

The Chairman: How does a crucible stand the melting of nickel?

Mr. Mixer: The ordinary plumbago crucibles that are used stand several melts of nickel if the nickel is melted without any fluxes; but if fluxes are used, they cut into the crucible; they loosen a little of the plumbago, and that seems to alloy with the nickel, and, in some cases, makes a carbide of nickel, or, at any rate, gives it a very dirty appearance. That is the case, of course, where fluxes are used that cut into the crucible; otherwise, a plumbago crucible should stand four or five melts of nickel. Nickel, as I suppose most of you know, is very hard to melt. I have forgotten whether the figures are a little above steel or a trifle below, but my impression is that they are a little above.

The Chairman: It used to be put down as one of the infusible metals, although I saw in one of the text-books the statement that nickel melts very easily.

Mr. F. Crabtree: The blow-pipe must have been used.

Mr. Mixer: Yes. Of course, we all know that one of the first things given the student of the blow-pipe to fuse is a very fine piece of platinum wire. Probably this man got hold of a fine piece of nickel and used the blow-pipe.

Mr. R. B. Carnahan: If we can't make use of nickel in any other way, we may substitute it for platinum crucibles.

Mr. Mixer: As to nickel crucibles, I believe some attempt is being made to make them; but, of course, they will not take the place of the platinum crucible. They would be very durable, however. A much better heat can be gotten than in porcelain for the incineration of such things as would not alloy with the nickel. Of course, platinum would be best, but I mean that the nickel may take the place of porcelain.

The **Chairman**: On the recommendation of a New York dealer, I ordered a dozen or more of nickel crucibles, and the first one I used met with a mishap; the bottom fell out the first time it was heated, and I have not used any since.

Mr. Eastwick: I would like to ask Mr. Mixer about Sperrylite, whether he has any samples of it, or knows anything about it?

Mr. Mixer: I have a sample of it, and I would have brought it along if I had thought of it. It is, as you know, an arsenide of platinum, found by Mr. Sperry, formerly chemist of the Canadian Copper Company, at, I believe, the Vermillion mine in the Sudbury district, where it is found in some quantity. It is found to be very hard to refine. Considerable of it, I believe, has been sent to England, and is being handled there, but I do not know of any of it being refined in this country, although I believe there is plenty here to justify its refining, if anybody can get hold of a suitable process.

Mr. R. B. Carnahan: I would like to ask the gentleman what method he finds best for separating nickel from iron in its ores?

Mr. Mixer: That is a question that has been puzzling me a good deal since I have been connected with the nickel company. I will state, that in a couple of weeks the *Engineering and Mining Journal* will have an article giving our method. Dr. Emmens has written an article on the subject which will doubtless prove interesting to those who have made nickel determinations. The basic acetate method, I believe, is the best; but still, it is considerable of a nuisance, in my opinion; and I found a very handy method for analyzing such things as pyrite and pyrrhotite where there is a large proportion of sulphide of iron and a very small proportion of sulphide of nickel. I dissolve it in aqua regia or nitric acid, and get it in solution, and then precipitate the iron with ammonia. That would leave a little nickel in solution, and, of course as usual, the hydrate carries down large quantities of nickel. That process could be repeated two or three times, either with ammonia or the basic acetate—the basic acetate being preferable. We found a handy little kink there, in this way: Studying up the theory of thermal units, we found that the heat of combination of a molecule of hydrate of nickel was greater than that of the combination of hydrate of iron and acid. Consequently, I take this precipitate of iron hydrate which holds some of the nickel, wash it into a dish, and just barely acidify it with sulphuric acid; then I would boil for, say, ten minutes. This, in theory (and, in practice, I found it so), would take out a large part of the nickel that was left in the precipitate, and, if an excess of acid is used, a little iron is taken up also, and then you have a solution which, of course, contains more nearly equal proportions, and ordinary separation with ammonia would be nearly correct. I have the figures of the thermal units here, which I will give you:

	Units.
$Fe + O + SO_3 + Aq$,	93200
$Ni + O + SO_3 + Aq$,	86950
$Fe + O + H_2O$,	68280
$Ni + O + H_2O$,	60840

From these figures, taken from Muir and Wilson's *Thermal Chemistry*, the heat of formation of one molecule of ferrous sulphate, by the union of ferrous hydroxide and dilute sulphuric acid, is $93200 - 68280 = 24920$, while that of nickel sulphate is $86950 - 60840 = 26110$ units.

Under these conditions, Berthelot's law of maximum work calls for the formation of nickel sulphate in preference to ferrous sulphate.

I found that to work in practice fairly well, and for a rough method for determining the nickel where there is a large quantity of iron present, I found it quite handy, although I should want to have a great many analyses made by it before I would be sure of its working in every case. In the case of analysis where the quantity of iron is very small, compared with the quantity of nickel, I find a very good way is to make a solution, get out what copper there is by hydric-sulphide, and then make the solution up to, say, something like 400 c.c., take an aliquot proportion of it, and precipitate both the iron and the nickel with soda or potash, then wash this precipitate, and dissolve it in dilute sulphuric acid into a platinum dish, make ammoniacal, and electrolyze. The iron immediately becomes oxidized and floats on top; and, it seems to me, a very good way for determining the nickel. I have not found that the coating of nickel on the platinum dish contains any considerable quantity of iron, and consequently it seems to me, that where the nickel is very high in proportion to the iron, that would be a very good method. For all ordinary analyses, I think it is all right. Of course, getting down to second and third place decimals it will hardly do.

MR. JAMES CAMP: I would like to ask why one of the specimens handed around to-night is called Bessemer matte?

MR. HANDY: I think that the Bessemer converter is used in refining matte.

MR. H. T. WEED: There is an occurrence of nickel which has not been spoken cf. Last summer, at the zinc mines in New Jersey, there was a deposit of niccolite found in the ore which consists mainly of Franklinite and Willemite. The niccolite occurred in grains, perhaps three or four millimeters in diamteer, and rounded almost as if worn by the action of water. So far as I know, that is the only deposit of nickel in New Jersey.

MR. MIXER: Nickel has been traced all along the Appalachian chain of mountains, from Georgia, through North Carolina, through Pennsylvania, as you said, on the eastern boundary, in New Jersey, and into New England. There seems to be a general trend of nickel all along the Appalachian chain, in very small quantities, of course.

I was amused the other day. We received a letter from Georgia stating that there was ore there containing 4 per cent. of nickel and 2 per cent. of cobalt. It was looked into, and samples sent me. I found the faintest trace of nickel, and found where the mistake was; we found that the man who wrote in the letter "4 per cent.," didn't mean it; he meant 4 pounds per ton, and that is about the character of the nickel ores in the Appalachian chain.

MR. EASTWICK: Can you say whether the Sudbury deposit continues down through the United States, or not?

MR. MIXER: I saw by the papers the other day, that in the vicinity of Duluth, Minn., nickel had been found. I suppose some day nickel may be found in the great copper region along the southern shore of Lake Superior.

SULPHUR IN COAL AND COKE.

RESULTS BY ESCHKA'S METHOD VS. THOSE BY HUNDESHAGEN'S METHOD.

BY JAMES O. HANDY.

Eschka's Method (Pittsburg Testing Laboratory practice): One gramme fine coal, 1 gramme pure MgO (Baker and Adamson's), and ½ gramme C. P. Na_2CO_3 (Baker and Adamson's) are mixed thoroughly and heated in a 100 c.c. platinum dish over a Bunsen burner. Begin with flame just touching dish. Raise heat till in fifteen minutes the dish is red-hot. Stir frequently with a platinum wire until carbon has burned away. Let cool. Brush contents of dish into a 200 c.c. beaker, add 100 c.c. of water, 15 c.c. of Bromine water, and boil for five minutes. Filter and wash well with hot water. Make filtrate up to 250 c.c., add HCl until just acid and boil off bromine; precipitate with $BaCl_2$ as usual. $BaSO_4 \times .13756 = S$. No sulphur has been found in B. and A., MgO or Na_2CO_3, and no correction is required. This method has been used by us practically in the present form for the past four years, and has given satisfaction.

Hundeshagen's method, as described in *J. A. and A. C.*, vol. vi., No. 7, p. 385, is essentially as follows: One gramme of coal and 2 grammes of a mixture of 2 parts pure MgO and 1 part pure K_2CO_3 are placed in a platinum dish and mixed. The remaining operations are practically the same as described above under Eschka's method. In our experiments on samples of coal from different sources, we obtained the same results by the Eschka (E) method and the Hundeshagen (H) method. We noticed no greater rapidity of burning in the " H " than in the " E " process.

SULPHUR IN COAL.

(Comparison of Eschka and Hundeshagen processes.)

No.	Source.	Fixed Carbon. Per cent.	Volatile Matter. Per cent.	Sulphur by " E." Per cent.	Sulphur by " H." Per cent.
10,170,	Westmoreland Co.,	55.18	33.40	1.17	1.19
10,516,	Pittsburg vicinity,	52.92	30.90	1.66	1.67
10,517,	Pittsburg vicinity,	57.37	31.62	1.77	1.77
8,825,	Unknown,	48.83	35.88	6.185	6.190
9,594,	Chartiers,	54.48	36.30	1.268	1.266
9,395,	Youghiogheny,	54.46	35.32	1.948	1.952

These experiments effectually disprove Hundeshagen's assertion that sodium carbonate in the Eschka mixture is not as efficient in retaining the sulphur of certain coals as is the K_2CO_3 in his mixture. It may be that none of the samples of coal which we examined contained any sulphur in organic combination. It seems hardly probable that such was the case.

DISCUSSION.

MR. JAMES CAMP: We tried those methods in the laboratory, and got practically the same results.

The CHAIRMAN: The two methods gave the same results?

MR. JAMES CAMP: Yes, sir; in an analysis of coke, there was only about .01 difference. There are some disadvantages in using carbonate of soda, and I have been using a " fusion " mixture for that work.

The CHAIRMAN: To take the place of carbonate of soda?

MR. JAMES CAMP: Yes, sir.

MR. R. B. CARNAHAN: I think very few chemists use the *fusion method.* Although it takes a little more attention, I think it is as rapid as the lime or magnesia method. I can make a complete fusion, and have the solution ready for acidifying and evaporating in ten minutes.

The CHAIRMAN: Have you found higher results by the method you use than by the *Eschka method?*

MR. R. B. CARNAHAN: I never found any difference. I fuse with the double carbonate of soda and potash. For one gramme of coal I use ten grammes of carbonate of soda and ten of potash, and one gramme of nitre. I may use an excessive amount of alkali; I am not sure about this; it gives good results, and checks well. The fusion method, I think, has always been considered the standard method. Dr. Drown so considered it.

The CHAIRMAN: I think the *Eschka method* compared very accurately with the Hundeshagen methods in the results detailed in one of the reports of the Berlin Testing Laboratory, applied especially to different kinds of coal. It was found that the *Eschka method* gave very correct results.

MR. JAMES CAMP: I would like to ask Mr. Handy what kind of coke that was with 1.87 per cent. of sulphur?

MR. HANDY: It was some very poor coke from Westmoreland county.

MR. JAMES CAMP: Where is that coal that contained 6.19 per cent. sulphur from?

MR. HANDY: Our clients gave us only a number.

INDEX.

Abbreviations.—"Ind.," Journal of Society of Chemical Industry.

"C. N.," Chemical News.

"J. A. and A. C.," Journal of Analytical and Applied Chemistry.

"J. I. and S. Inst.," Journal of the Iron and Steel Institute.

"A. I. M. E.," Transactions American Institute Mining Engineers.

REFERENCES TO RECENT ARTICLES ON SUBJECTS DISCUSSED AT THE MEETING, OCTOBER 25TH, OF THE CHEMICAL SECTION.

Nickel, Mining and Metallurgy of.

"Nickel and Copper Deposits of Sudbury," "Ind.," 1891, p. 372; "A. I. M. E.," vol. xviii., p. 278.

"Nickel Carbon Oxide (Chemical Properties of)," "C. N." 8, 28, '92; "Ind.," 10, 31, '91.

"Nickel Ores of New Caledonia," "C. N.," 10, 10, '90.

"Nickel Extraction of Ores," "A. I. M. E.," vol. x., p. 305.

Nickel Alloys.

"Nickel and Steel, Alloys of," James Riley, "J. I. and S. Inst.," Vol. I., 1889.

Nickel Determination in Ores and Alloys.

"Nickel, Separation from Iron and Cobalt," "C. N.," 4, 24, '91, 1194.

"Nickel, Separation from Cobalt," "C. N.," 5, 29, '91, 6, 5, '91, and 6, 12, '91.

"Nickel, Separation from Cobalt," "C. N.," 10, 30, '91; "Ind.," 4, 30, '90; "C. N.," 12, 11, '91.

"Nickel Separation from Zinc," "Ind.," 4, 30, '89, p. 307; 3, 31, '90; and "C. N.," 3, 14, '90.

"Nickel and Copper Determination in Ores and Furnace-Products," by David H. Browne, "J. A. and A. C.," April, 1892.

Sulphur in Coal.

"Estimation and Occurrence of Sulphur in Coal" (a comparison of results by different methods of analysis), by G. H. Bailey, "Ind.," 5, 31, '89.

Adjourned at 10 P.M.

JAMES O. HANDY,
Secretary.

NOVEMBER 15TH, 1892.

THE Society met in the Parlors of the Academy of Science and Art on November 15, 1892. Mr. Alfred E. Hunt, President, in the chair; Mr. R. N. Clark, Secretary; seventy-two (72) members were present.

The minutes of the last meeting were read and approved.

The following were elected to membership: H. H. Rousseau, Alex. Nurich, Frank T. Oakley, R. T. Hatch, R. H. Proberst,

M. E., J. Harkins, James K. Lyons, Robert Gray, Edwin H. Beazell, James Mackenzie, George S. Page.

On motion the following members were elected by the Society to nominate officers for the ensuing year and to report at the December meeting: Messrs. W. Lucien Scaife, William Metcalf, Geo. S. Davison, Charles Davis, Alexander Dempster.

On motion the Society agreed to have an annual dinner at some date in January next. The chair appointed the following committee: Messrs. Harry J. Lewis, Emil Swenssen and Gustave Kauffman.

Mr. W. Lucien Scaife then read the

REPORT OF THE COMMITTEE ON SMOKE PREVENTION.

Your Committee, appointed at the March meeting of the Society, have had neither the time nor the opportunity to make a detailed or experimental investigation of the general subject of smoke abatement. We believe we have, however, obtained sufficient facts, directly bearing on our local conditions, to show that there is in this respect a chance for considerable improvement at the present time in Pittsburg and Allegheny. As the subject is one of general interest, and as all permanent smoke abatement is dependent on the public demand for it, you will probably pardon us for giving some historical and technical details with which most of you are familiar.

HISTORICAL.

It is a matter of common remark, that the few years of comparative freedom from coal smoke, due to the use of natural gas, have given the people of this community a strong desire to avoid the evils now menacing us from the rapidly increasing consumption of soft coal. It is therefore not very surprising that the English people of the early part of the fourteenth century should have enacted a law making it a capital offense to burn coal and pollute the atmosphere within the precincts of London.

Over three centuries were required to remove the prejudice of the English people against the use of soft coal. It is related that an ambassador at Paris, early in the present century, sent out invitations for a large party, but was astonished to find that none but gentlemen attended. The ladies declined to come because they had heard that his lordship used English coal to warm his house. At the present day in Paris very little coal is used, the price of gas coke being kept so as to make it cheaper than coal. The result is that while Paris, even in rainy and foggy weather, is well lighted during the day, London, at noon, is sometimes shrouded in the darkness of night.

Nevertheless, during the present century, England has done considerable to abate the smoke nuisance. In 1821 an Act was passed by Parliament, " For giving greater facilities in the prosecution and abatement of nuisances arising from furnaces and steam engines." It empowered the court before which the case was heard, to judge and award suitable costs to the prosecutor, and likewise to direct the remedy which should be applied to the furnace, to prevent a repetition of the offence. As, however, it made no provision for official inspection, and left the prosecution to private individuals, it became practically inoperative, although in force thirty-two years.

Again in August 1853 Parliament passed a bill " to abate the nuisance arising from the smoke of furnaces in the metropolis, and from steam vessels above London Bridge." This Act, unlike the former, applied only to London. It provided for official inspection under the authority of the Commissioners of Police. The fine for the first offence was forty shillings, being doubled for each subsequent conviction. The Act further provided that the words "consume or burn the smoke," should not be held in all cases to mean " to consume and burn all the smoke," but allowed the justices to remit the penalties in cases where the defendant "consumed or burned" his smoke *as far as possible under the circumstances.*

As illustrating the great advantage of placing the execution of such a law in the proper hands, we may add that the inspector appointed under this Act discharged his duties with such zeal and efficiency, that, in the words of our English historian, " A clean shirt, which formerly lasted only one day, now lasts four days in good condition." Parliament was so far satisfied with the results obtained in the metropolis under the Act, that, in the Sanitary Act of 1866, clauses were inserted providing for the extension of its benefits over the whole kingdom, by empowering local authorities to take measures for the suppression of the smoke nuisance and allowing twelve months for manufacturers to comply with the provisions of the law. Many towns availed themselves of this law and sought to give it effect by appointing smoke suppression committees and salaried inspectors. Cities such as Birmingham and Glasgow have had considerable success in this direction. But owing to the little progress made hitherto with domestic fires, very large and densely populated cities like London still suffer from coal smoke. Some years ago, an exhibition of domestic grates and stoves was held at South Kensington,—careful tests being made as to economy and smoke production. But no important improvements appear in the reports as far as we are aware.

About a century ago, Benjamin Franklin was perhaps the best known expert on smoke production and prevention. Although his writings deal more with "smoky" than with "smoking" chimneys, yet he describes two "smoke consuming" stoves designed by himself. One was shaped like a vase placed on a pedestal. It was completed in 1771, and satisfactorily used by him for three winters in London, and one winter afterwards in America. It had a downward draft through the grate, and was practically smokeless. But it evidently required an expert to kindle a fire in it without filling the room with smoke. This difficulty led Franklin to design another stove which although not so smokeless as the preceding was more practical, and may contain the germ of future smokeless stoves and fire-places. It consisted essentially of a cylindrical grate suspended in a fireplace, and movable about a horizontal and a vertical axis. Franklin thus describes its use: "In making the first fire in a morning with this grate, there is nothing particular to be observed. It is made as in other grates, the coals being put in above, after taking out the upper bar and replacing it when they are in. The round figure of the fire when thoroughly kindled is agreeable; it represents the great giver of warmth to our system. As it burns down and leaves a vacancy above, which you would fill with fresh coals, the upper bar is to be taken out, and afterwards replaced. The fresh coals, while the grate continues in the same position, will throw up as usual a body of thick smoke. But every one accustomed to coal fires in common grates must have observed that pieces of fresh coal stuck in below among the red coals have their smoke so heated as that it becomes flame as fast as it is produced, which flame rises among the coals and enlivens the appearance of the fire. Here then is the use of this swivel grate. By a push with your tongs or poker, you turn it over on its axis gently till it again faces the room, whereby all the fresh coals will be found under the live coals, and the greater part of the smoke arising from fresh coals will in its passage through the live ones be heated so as to be converted into flame; whence you have much more heat from them and your red coals are longer preserved from consumption.

It may with a touch be turned more or less from any one of the company that desires to have less of its heat, or presented full to one just come out of the cold. And supported in a horizontal position, a teakettle may be boiled on it."

Since Franklin's time, our manufactures and population have grown enormously, and with them the use of coal and the production of smoke. Yet until recently no successful efforts appear to have been made, on a large scale, to abate the production of smoke with its attendant evils. Chicago and Cincinnati have passed ordinances forbidding the emission of black smoke except from dwellings. St. Louis is seriously considering the question, but has as yet taken no legal steps.

The present agitation of the smoke question in Pittsburg is due to the efforts of the Women's Health Protective Association of Allegheny County, in the hope that the benefits of a comparatively unpolluted atmosphere enjoyed during the use of natural gas, may not be lost as that valuable gift of nature disappears.

GENERAL PRINCIPLES GOVERNING THE COMBUSTION OF COAL AND THE PRODUCTION OF SMOKE.

Combustion is a chemical process, which often takes place by the ordinary operations of nature and without human interference, as in the case of a forest which has been set on fire by a flash of lightning. But the conditions which hinder or promote this chemical action are numerous; they are chiefly mechanical in character, and to this extent are largely controllable by man. A consideration of the principles which enter into the phenomena of combustion may therefore be divided into two classes:

First.—Chemical. Second.—Mechanical accessories.

Chemical Principles.

All commercial fuels contain the elements carbon and hydrogen as essential constituents, and in addition others, chiefly mineral, which enter only indirectly into the process of combustion, usually with the result of retarding it. They form the ash of the fuel, and hence are objectionable but unavoidable portions of it.

Carbon is a solid. During combustion it becomes incandescent, and glows after the familiar manner of a charcoal or a coke fire; and when combustion is rapid and complete, it does not yield any visible flame.

Hydrogen is a gas. It burns always with a flame absolutely free from smoke, but its incandescence is so slight that in bright daylight it is very difficult to see the flame which, indeed, emits only a faint bluish light in a darkened room.

It follows from the above statements that hydrogen is the flame-producing ingredient of fuels, while carbon is the incandescent or light-giving one. For practical purposes, fuels may be divided into those which burn with very little flame or smoke, and those which burn chiefly with a flame, and which may emit much smoke. The smokeless fuels are charcoal, coke and anthracite coal; these contain very little hydrogen, and usually upwards of 80 per cent. of their weight is carbon. The flaming fuels are wood, bituminous coal, petroleum, pitch, natural gas and all forms of oils and fats whether derived from fishes, animals or vegetables. In these the hydrogen may range from 8 to 25 per cent.

Combustion is a process of oxidation, and consists in a chemical union between the carbon and hydrogen of the fuel with the oxygen of the air. Hydrogen and oxygen unite to form water. Carbon may unite in two proportions, forming two oxides called respectively carbon monoxide and carbon dioxide. This latter is often called carbonic acid, and represents the final result of the complete combustion of carbon. These products are fixed gases, and, since at the temperature of the flame water can only exist as steam, we may consider all the products of the complete combustion of a fuel as being invisible gases, which escape into the atmosphere. A jet of illuminating gas burning in an apartment very nearly realizes this condition.

But the hydrogen and carbon of a fuel are not simply mixed together; they are combined, forming a class of compounds known as hydrocarbons, of which natural gas and petroleum are familiar examples.

Hydrogen ignites at a lower temperature than carbon. Moreover being a gas it mixes readily with the air, and hence is more combustible than carbon. When a hydrocarbon is heated to a temperature considerably above a red heat it begins to separate into gaseous hydrogen and solid carbon, and the chemical oxidation which constitutes combustion cannot take place until this preliminary separation begins. It will easily be seen that if the supply of air is ample, both the hydrogen and carbon will burn completely into carbon dioxide and water, and only odorless and invisible products of combustion will result; but if the air supply is not ample, or what amounts to the same thing, if the air is not nearly instantaneously mixed with the vapor of the hydrocarbon, then the hydrogen, being more combustible than the carbon, will take most of the oxygen present and leave the carbon in the form of small particles, which, while hot, radiate light, but which soon float outside the borders of the flame, cool down and appear as soot and smoke.

Improper supply of air is then the sole cause of the production of black smoke. But there is another kind, a light yellow smoke which is produced from another cause.

If oil or fat is dropped on the top of a stove which is barely red hot much thin white smoke will result, but there will be no flame and no combustion; this is because the oil is vaporized, or, in technical language, suffers destructive distillation. All the elements of the oil are still present, and if a lighted match is applied to the column of smoke it will ignite and burn with a bright flame. Similarly when bituminous coal is charged on the top of a fire, it cannot all be heated instantaneously, but portions of it will suffer destructive distillation as it gradually approaches a red heat, and a thick yellow smoke will be given off from all parts of the freshly added fuel where the temperature is not high enough to ignite it. This yellow smoke consists mainly of small globules of oily and tarry bodies which give to coal smoke its adhesive and greasy character.

If we could always have a flame playing over all portions of the top of a fire, and at the same time an ample supply of hot air thoroughly mixed with the fuel gases, there would never be any notable quantity of either yellow or black smoke passing up the chimney. Theoretically, the complete abolition of smoke can be secured by having a top flame and a top supply of hot air. But just here is where the practical difficulties begin; for however ample may be the admission of air to the ash-pit, and however well cleared may be the surface of the grate-bars, the passage of air through four or five inches of incandescent fuel completely deprives it of free oxygen, so there is none available to ignite the freshly-charged coal. But air admitted simply through slits in the fire door, while helping to keep up a top flame, yet being cold and imperfectly distributed, will chill some portions of the fuel gases below the igniting point, and consequently imperfect combustion, with the production of black smoke, will result. Hence an essential requirement of devices for smoke-prevention is *an ample, timely and thorough* MIXTURE *of air with the combustible gases distilling from the fuel.*

MECHANICAL ACCESSORIES.

The style of furnace known as the "regenerative" is the best-known plan for pre-heating the air which is to be supplied to the fuel through the air-door. It, however, is not suited for boilers and for many other applications of fuel. The former are probably the greatest smoke-producers in Pittsburg and Allegheny, and are daily becoming more numerous owing to the increasing use of steam for elevators, heating, lighting, and power in buildings throughout these cities. Much of their smoke is due to badly-constructed furnaces and to the fact that the boiler capacity is insufficient to properly fulfil the duty required of them. Their fires must, therefore, be urged beyond the capacity of the grate-surface and combustion-chambers, resulting in the dense volumes of smoke which we daily see pouring out of stacks and chimneys all over the city.

A great number of devices have been proposed for the purpose of increasing the economy of combustion of fuel and diminishing the quantity of smoke produced. We cannot attempt to describe these in detail, but will try to give the principal characteristics of the most prominent classes at present in use.

BRICK ARCHES AND CHECKER-WORK.

In order to pre-heat the "top" air before it reaches the fuel-gases, it is sometimes admitted through openings in the brick-work, at the side of the fire, or through slits in the fire-bridge. This is a partial remedy only, because the air is rarely sufficiently heated or mixed thoroughly enough with the distilling products of the coal.

Arches of firebrick are sometimes placed near the front end of boilers and help to mix the air and gases, to store and reflect heat, and to direct the mingled gases upon a bed of glowing coals kept at the rear end of the furnace. There are several varieties of these arched furnaces in use at present. The best of them, when properly handled, are economical in fuel, produce but little smoke, and seem destined to meet especially the wants of small plants where automatic stokers are not applicable or desirable. Their first cost is not great, but the arches must be made properly, or they will not be durable. Furnaces thus equipped make very little smoke when a good fire is kept up, but they will produce considerable smoke if the fire is urged too much or allowed to run too low.

AUTOMATIC STOKERS.

These are mechanical arrangements, more or less complicated and costly, designed to supply the fuel—not intermittently, but constantly—and to spread it in thin layers over the fire. Thus the temperature need not be lowered below the igniting-point, and the duration of the period of destructive distillation, which precedes ignition, is made very brief. There are several very good stokers now on the market which accomplish the desired result with greater or less success, according as they are permitted to feed the coal slowly or rapidly. If the feeding is made slow enough, the suppression of smoke may be theoretically complete. But to feed slowly means that a boiler must not be crowded. Hence the use of a mechanical stoker and the suppression of smoke may necessitate an increase of the number of boilers to do the work formerly done under hand-stoking, with the generation of smoke. That is, the mechanical stoker, to be always effective, will often require the manufacturer to put a little more money into his plant. Smoke must be paid for, in one way or another. If it is suppressed, it represents the interest on a few extra thousand dollars of plant. If it is produced and poured out into the air, then the public pays the bill in the form of a greater wear and tear on clothing and household property, as well as in the general depressing effects of a smoke-laden atmosphere.

Mechanical stokers are especially adapted for large plants, where they can be so arranged as to displace manual stoking. Their cost will prevent their adoption in many small plants, although here and elsewhere numbers have been applied to single boilers with satisfactory results. Where either vertical or horizontal space is restricted, it is sometimes impossible to apply mechanical stokers. Like all plans of successful firing, they require intelligent manipulation, or they will smoke and give trouble. Pittsburg coal, in the form of nut or slack, is well adapted for automatic stokers, some of the principal types of which are represented here. One of their advantages is that an inferior fuel can be successfully burned by them, but not always to the extent claimed by their advocates.

STEAM JETS.

There are in use a number of devices by which jets of steam are arranged to force currents of air above or below the fire. They are simple in construction, inexpensive in first cost and repairs, require very little space, and can be readily applied to existing furnaces. The best of them, even when slack is used, can reduce to an almost invisible vapor, in less than half a minute, dense black smoke from a boiler-fire driven to the utmost. One of the most varied and successful applications of steam jets may be seen at a well-known mill in this city. After several years of use of natural gas, the owners were forced to return to coal. They found, however, that with coal their boilers would not supply sufficient steam. Accordingly they concluded to try steam jets to increase combustion and evaporation. After some experimenting, they finally adopted a jet somewhat of the Bunsen-burner type, which is now in successful operation on two batteries of flue-boilers, the jets being placed above the fire-doors and immediately below the boilers. The fuel is slack, and yet almost no smoke is visible, even during heavy firing. The same firm have also applied to 18 puddling and 4 heating furnaces steam jets of a different design. In these the ash-pit is made tight and the steam and air enter below the grates. These furnaces are of the usual type, except that special openings are made for air above the fire and through the bridge wall. When the fire-doors are closed no black smoke is visible while the steam jets are in operation. The smoke appears above the stack whenever the fire-door is opened, but disappears immediately on closing the door. Each puddling furnace has 2 jets of steam, and each heating furnace 5 jets, all about $\frac{1}{10}$ inch in diameter. Nut coal can now be used in the heating furnaces and slack in the puddling furnaces, whereas lump was used formerly in both. The jets have been in use for nearly a year, and the mill owners state that they have not injured the boilers, nor the furnaces, nor the iron, but have saved money by reducing the quantity and quality of the fuel required. Unfortunately, the steam jet makes a great noise. This is not very objectionable in a rolling-mill or machine-shop, but unless suppressed will probably prevent its introduction into office buildings, where otherwise it might be useful. We may add that some skill is required to properly operate steam jets, as is the case with all other smoke-preventive devices.

OTHER MEANS EMPLOYED.

Lima oil is at present used in various kinds of furnaces, being fed into the combustion-chambers by means of air under considerable pressure. It makes an easily-regulated, smokeless fire, but has not received much application in Pittsburg owing to its cost being equal to or greater than coal. Its disagreeable odor is also an objectionable feature, as well as the fact that it increases insurance risks.

Producer-gas is largely used in regenerative furnaces, where it burns with little or no smoke. It is not economical under boilers.

Electricity is proposed for heating purposes; but at present it finds little or no application here. It is an ideal smokeless agent and may in the future aid to solve the problem of domestic smoke now so menacing, with the gradual disappearance of natural gas. In the shape of lights and motors it has already displaced many smoke producers.

PITTSBURG'S SMOKE PROBLEM.

Every city has its peculiar needs and activities, which must be recognized in attempting to apply to it general results applicable elsewhere. Of no city is this more true than of Pittsburg, with its varied industries, its natural resources and enormous production. Hitherto, smoke has been the black ensign of the ceaseless warfare carried on here with the forces and materials of nature. Can we destroy or reduce the ensign without injury to the best interests of our city? The question is not a simple one, as has been well shown by recent papers and discussions of this Society.

It is estimated that before the introduction of natural gas Pittsburg consumed about 10,000 tons of coal daily, and that the present consumption is about 7000 tons. In Pittsburg and Allegheny about 25,000 houses and many manufactories are still supplied with natural gas, amounting to several hundreds of millions of cubic feet daily, and probably representing more than half the present coal consumption.

If therefore our skies are darkened now, what may we expect when the present almost smokeless natural-gas fires become smoke producers? Our finest residence district, the East End, will especially suffer by the change. Already, owing to the peculiar topography of Pittsburg, and in spite of the special smoke ordinance recently passed, the East End is often covered by clouds of smoke, mostly produced in the lower parts of the city and by passing locomotives. Of the 10,000 houses probably occupying the district, it is estimated that four-fifths use natural gas at present. Were the latter to return to coal, and the manufactories to their former smoke emission, we can readily imagine the increased blackness of the skies; but we cannot estimate all the material, mental and physical evils which would ultimately follow. Allegheny would likewise suffer, though possibly to a less extent. We must confess that up to the present time, outside of coke or a gaseous fuel, there has appeared no practical solution of the smoke problem in dwellings, although with a return to soft coal they will increase the quantity of smoke as the city grows. It is therefore all the more important that other sources of heat should be rendered smokeless as far as possible.

Steam boilers are probably the principal cause of smoke in the city. But there appears to be no reason why they should not be rendered almost smokeless except for a few minutes daily. As many are put in buildings erected in the heart of the city, where their fires emit dense volumes of smoke, because no adequate provision is made by the architects or builders for proper stoking apparatus, furnaces or flues, we believe that building inspectors should in the future be instructed to see that the needs of smokeless combustion are attended to, at least in so far as sufficient flue and furnace space are concerned.

Puddling and heating furnaces are not very large smoke producers in this locality, and steel melting furnaces still less so. Moreover, puddling furnaces are rapidly disappearing. Hence there is not the same necessity as with boilers to enforce the adoption of smoke-preventive devices, especially if such devices tend to injure the product. But in view of the facts mentioned above, it would seem proper to require such furnaces to be so managed as to restrict the black smoke to occasional appearances only. In such furnaces it is, of course, understood that an excess of unburnt carbon is required in the atmosphere above the hearth in order to prevent oxidation of the iron. But only a slight excess of carbon is necessary, as is confirmed by the experience of this city during the natural gas period, because for nearly four years the heating and puddling furnaces, fired with gas, emitted very little smoke.

Locomotives and steamboats are important smoke producers here. The latter have furnaces which can be treated like those of ordinary stationary boilers. Hence their smoke can be abated readily. Improved steam jets have been successfully used on Western locomotives, and one device has been approved of by the Chicago Society for the Prevention of Smoke. In the latter, steam jets draw air from the ash pit below the grate and force it above the fire, both front and back; while a circle of jets placed in the stack, increase the draft. Locomotives should therefore be required to suspend black smoke emission while in the city limits,—as is the case at present in Allegheny.

CONCLUSIONS AND RECOMMENDATIONS.

It will be evident from the foregoing remarks, that we cannot hope to free our city from all the smoke now poured from stacks and chimneys. But it is within the range of present possibilities to abate the greater part of the nuisance. To this end we would recommend:

1. That the Women's Health Protective Association or some similar organization, continue their efforts toward smoke prevention by educating the community in its principles, and advocating the use of smokeless fuels in dwellings, and the best stokers or other devices in manufactures and steam plants.

2. That our City Councils should pass an ordinance for the abatement of the smoke nuisance, insisting on the absence of dense smoke from stationary, steamboat and locomotive boilers, except when fires are started, but recognizing the necessities of puddling and other furnaces which require a small excess of carbon for proper working.

3. That one of the duties of building inspectors or of persons appointed for the purpose, should be to see that newly erected

buildings have properly designed flues and ample room for furnaces, with particular reference to economical combustion and the non-emission of smoke.

APPENDIX I.

Smoke Ordinance of the East End, Pittsburg.

AN ORDINANCE—To regulate and suppress the production and emission of smoke from bituminous coal, and to provide penalties for the violation thereof in certain districts of the city.

SECTION 1. *Be it ordained and enacted by the City of Pittsburg:* That from and after September 1, 1892, the emission of smoke from bituminous coal or escape of such smoke from any chimney or smoke stack used in connection with stationary boilers within the limits of the following defined district: Beginning at Miltenberger Street and Second Avenue; thence along the northern curb of Second Avenue to the eastern line of the city; thence along said city line to the Allegheny Valley Railroad; thence along the Allegheny Valley Railroad to the Sharpsburg Bridge; thence along Bridge Street to Butler Street; thence along the southern curb of Butler Street to Penn Avenue; thence along the south curb of Penn Avenue to Thirty-third Street; thence along Thirty-third Street to Center Avenue; thence by Center Avenue to Devilliers Street; thence by Devilliers Street to Dinwiddie Street; thence by Dinwiddie Street to Fifth Avenue; thence by Fifth Avenue to Miltenberger Street; thence by Miltenberger Street to Second Avenue, at the place of beginning, shall be deemed and is hereby declared to be a public nuisance.

SEC. 2. That it shall be unlawful for any corporation, co-partnership or individual owning, controlling or using any chimney or smoke stack used in connection with stationary boilers within the limits of the district, as provided in Section 1, to allow, suffer or permit smoke from bituminous coal to be emitted or to escape therefrom.

SEC. 3. Any corporation, co-partnership or individual who shall or may allow, suffer or permit smoke from bituminous coal to be emitted or to escape from any chimney or smoke stack used in connection with stationary boilers within the limits of the district, as provided in Section 1 as aforesaid, shall, in addition to any and all law requiring the abatement of nuisances, forfeit and pay to the city of Pittsburg for every such offence a sum not less than ten dollars or more than fifty dollars, to be recovered before any Alderman of the county of Allegheny or any Police Magistrate of the city of Pittsburg as debts of like amounts are now recoverable.

SEC. 4. No discrimination shall be made against any device or method which may be used which will accomplish the purpose of this ordinance in relation to the said matter.

SEC. 5. The Chief of the Department of Public Works of the city of Pittsburg is hereby empowered to enforce the provisions of this ordinance.

APPENDIX II.

Chicago Smoke Ordinance.

SECTION 1650. The emission of dense smoke from the smoke stack of any boat or locomotive, or from any chimney anywhere within the city, shall be deemed and is hereby declared to be a public nuisance, *provided* that chimneys of buildings used exclusively for private residences shall not be deemed within the provisions of this ordinance.

SEC. 1651. The owner or owners of any boat or locomotive engine, and the person or persons employed as engineer or otherwise, in the working of the engine or engines in said boat or in operating such locomotive, and the proprietor, lessee or occupant of any building who shall permit or allow dense smoke to issue or be emitted from the smoke stack of any such boat or locomotive, or the chimney of any building within the corporate limits, shall be deemed and held guilty of creating a nuisance, and shall for every such offence be fined in a sum not less than five dollars nor more than fifty dollars.

SEC. 1652. It shall be the duty of the Commissioner of Health and the Superintendent of Police to cause Sections 1650 and 1651 of this article to be enforced, and to make complaint against and cause to be prosecuted all persons violating the same.

APPENDIX III.

Cincinnati Smoke Ordinance.

SECTION 1. Be it ordained by the Board of Legislation of the city of Cincinnati, that the emission of dense smoke from the smoke stack of any boat or locomotive, or from any chimney anywhere within the city, shall be deemed and is hereby declared to be a public nuisance, *provided* that chimneys of buildings used exclusively for private residences shall not be considered within the provisions of this ordinance.

SEC. 2. The owner or owners of any boat or locomotive engine, and the person or persons employed as engineer or otherwise in the working of the engine or engines in said boat or in operating such locomotives, and the proprietor, lessee or occupant of any building who shall permit or allow dense smoke to issue or be emitted from the smoke stack of any such boat or locomotives, or the chimney of any building within the corporate limits, shall be deemed and held guilty of creating a nuisance, and shall for every such offence be fined in a sum not less than five dollars nor more than fifty dollars.

SEC. 3. It shall be the duty of the Supervising Engineer to cause the enforcement of this ordinance, and to make complaint against and cause to be prosecuted all persons violating the same; and in so doing he shall be assisted by the Superintendent of Police and the Health Officer, and their respective departments.

SEC. 5. This ordinance shall take effect and be in force from and after the earliest period allowed by law.

Passed July 15, A.D. 1892.

The Supervising Engineer of Cincinnati adds the following remarks:

"If you should pass this ordinance, you ought to amend it by inserting after the word 'occupant' the following words: 'Or the person or persons employed as engineer or otherwise in the working of the furnace or furnaces, or the person having control of the furnace or furnaces in the building.'

"I deem it the best law upon the subject that can be devised.

I refer you to the case of Harmon *vs.* Chicago, 110th Illinois, p. 400, for litigation sustaining this ordinance."

APPENDIX IV.

Cleveland Smoke Ordinance of February 2, 1892.

SECTION 1. Be it ordained by the Council of the city of Cleveland that the emission of dense smoke from any chimney anywhere in the city, or from the smoke stack of any boat, locomotive or stationary engine or boiler within the limits of the city of Cleveland shall be deemed and is hereby declared to be a public nuisance.

SEC. 2. That the emission into the air of any noxious gases or vapors or offensive odors from any manufactory, building or premises within the limits of said city, injurious to health or annoying to the inhabitants of said city, or any part thereof, shall be deemed and is hereby declared to be a public nuisance.

SEC. 3. That the owner, lessee, engineer, fireman, or other person or employee having charge or control of, or operating any boat, locomotive or stationary engine, or boiler, manufactory, building or premises, within the corporate limits of said city, who shall cause or permit dense smoke to issue from any smoke stack or chimney thereof, or connected therewith; or any owner, lessee, or other person or employee having charge or control of, or operating any manufactory, building or premises within the said corporate limits, who shall cause or permit any noxious gases or vapors, or offensive odors, as described in Section 2 of this ordinance, to issue from such manufactory, building or premises, shall be deemed and held guilty of creating a public nuisance, and upon conviction thereof in the police court of said city, shall be fined not less than ten ($10) dollars, nor more than one hundred ($100) dollars for each and every such offence.

SEC. 4. That it shall be the duty of the Health Officer of the Department of Police to enforce the provisions of this ordinance, and he shall, upon his own motion, or upon the complaint or information of any resident of said city, commence proceedings in the police court against any person for violating any of its provisions, whenever, in the judgment of said Health Officer, the facts warrant such proceedings.

SEC. 5. That the words "owner," "lessee" and "person" occurring in this ordinance in the singular number shall include the plural; and they shall also each be construed so as to embrace corporations, or partnership associations, as well as natural persons.

SEC. 6. That this ordinance shall take effect and be in force from and after its passage and legal publication.

APPENDIX V.

Birmingham (England) Act for the Prevention of Smoke.

1. Every furnace or fire-place used within the borough in the working of engines by steam or in any mill, factory, dye-house, brewery, bake-house, gas-works, or in any manufactory whatsoever, though a steam-engine be not used therein, and every steam-engine used within the borough, shall be so constructed as to consume the smoke arising from the combustibles used in such furnace, fire-place or steam-engine.

2. If any such furnace, fire-place or steam-engine not so constructed as aforesaid be used within the borough, or if any such furnace, fire-place or steam-engine so constructed as aforesaid be so negligently used as not to consume the smoke arising from the combustibles used therein, the owner or occupier of the premises whereon the same is used shall be liable to a penalty not exceeding five pounds, and to a further penalty not exceeding forty shillings for every day during any part of which said furnace, fire-place or steam-engine shall be so used after fourteen days' notice has been served by the corporation on the owner or occupier of the premises whereon the same shall be so used to remedy or discontinue the use of the same.

Provided that the words "Consume the smoke" shall not be held in all cases to mean consume all the smoke; and that a court of summary jurisdiction may decline to convict or may remit the penalties enacted by this section if the court is of opinion that the furnace, fire-place or steam-engine has been so constructed or altered as to consume as far as possible all the smoke arising therefrom, and has been carefully attended to and has consumed as far as possible the smoke arising therefrom.

Respectfully submitted,
THOS. P. ROBERTS,
Chairman.
THOS. H. JOHNSON.
CHARLES HYDE.
DANIEL ASHWORTH.
J. P. LANGLEY.
ALFRED E. HUNT.
W. LUCIEN SCAIFE,
Secretary.

DISCUSSION.

MR. A. E. HUNT: My impressions are that one of the most important things to correct in this matter of smoke abatement is the lack of steam capacity to do required work. That is especially applicable to the smaller plants and the places where steam is generated in amounts of one hundred horse-power or less. Few of the large mills have a deficiency of horse-power to the extent that would make an excess of smoke from that cause, because it has been found that it was a foolish and an uneconomical thing to do. But with the use of steam in the small plants which are being used for elevators and for every form of minor power in the city, there are very many cases where the provision of increased capacity is one of the things that will assist very greatly in the diminution of smoke.

The general statement of the committee is one that should be borne in mind, that to prevent smoke chemically speaking, it means an intimate mixture of the carbon and hydrogen of the fuel with a sufficient amount of heated air at the times and at the places where the combustion can properly take place. If these conditions are first understood and then devices are worked up along this line undoubtedly more successful appliances will be made in the future than have been in the past. Very many of the appliances that have so far been tried have been based upon wrong principles. In the future it must be understood that a successful appliance accomplishes the intimate mixture of the gases produced from the fuel with a sufficient apply of air at the temperature necessary for complete combustion.

MR. T. P. ROBERTS: This report is very broadly written; it states that smoke can be abolished in the business part of the city. But what I would like to speak about is in regard to the smoke of our domestic fires. Our report, I regret to say, and I gave that portion of it a great deal of study myself, gives but a hurried view. Speaking especially of the East End district there is an area there of about five square miles from Black Horse Hill to Fifth Avenue on one side and from Herrons Hill to Wilkinsburg on the other, which is well known to be the dwelling part of Pittsburg. It is a valley elevated about 250 feet above the river, enclosed by hills in all directions so that it does not receive the benefit of the ordinary river breezes which sweep over the lower portion of the city.

The conditions of the East liberty valley are very much like those of London, with its natural fogs and with smoke tending to combine with the fog. In this chosen part of Pittsburg there soon will arise an urgent demand for the application of contrivances to abate smoke made by domestic fires, and in my opinion the smoke problem will not be half settled until such contrivances are introduced. I do not regard it as practical to change the form of our fuel. Like the people of London Pittsburgers are in love with open grate fires, smoke or no smoke. An allusion was made in the report to some experiments made at the Kensington exhibition held in London some six years ago. There were sixty devices tested for domestic fires. While the reports, so far obtained for our committee, are not in detail, the general result was more favorable to a side draft or down draft for open grate fires.

MR. J. A. BRASHEAR: I have been interested in this problem since reading the writings of Count Rumford who did, as it is supposed, so much for smoke consumption and the perfect combustion of fuel in the houses of London and I investigated the London grates when I was there last spring. I am sorry to say that they were the worst possible in my opinion, for the proper consumption of fuel without smoke; they yield but little heat. I was asked by Mr. Scaife, a member of the committee, to see while over there what was being done in the line of smoke consumption. I confess that I went around London a good many days trying to find a smoke burner but I did not find any in the houses and I think they are far worse off in their methods of burning fuel than we are.

I think the consumption of natural gas and its diminution of late has led to improved processes. I know now that we are burning under our little ten-horse power boiler two-thirds less natural gas and producing better results than when we commenced, six years ago. I think the engineers here present, of whom I see some who are very eminent in their profession, agree that every rolling mill man puts up certain boilers to do certain work. He adds to the work, puts up new rolls and new machinery, but keeps the same old boilers.

MR. TYLER: I have given a great deal of attention within the last few years to smoke consumption and tried it in a number of ways. First I thought by getting a hot-air flue in the bridge wall, that would overcome the smoke, but it did not work. We afterwards tried having the flues around the side wall and that did not work. Then we went to the top of the boiler, and put our flues in there; that did not work. Then we tried some in both the side wall and the top; we had some result from that but nothing to speak of. I have now an invention which I think is equal to anything in the shape of a smoke consumer. I have an air jet and a steam jet which come to the front of the boiler just above the fire door and it makes no difference how much you force the fire no smoke comes from the stack.

MR. HUNT: Do you pre-heat your air in any way?

MR. TYLER: No, sir; it is cold air. I have placed four jets to the boiler.

MR. WM. A. HERRON: A week or two ago I went to the Thirteenth Ward pumping engine, for the purpose of seeing the smoke consumer and what it was doing. The engineer showed me the plant. It was a Roney stoker. He told me it was working admirably; there was no smoke coming from the stack and he said it saved about 33 per cent. in fuel. He was burning the worst kind of coal, even worse than slack. It was nearly dirt. He told me if he could get nut coal he would have no difficulty in saving a larger percentage. I noticed the effect because we have had so much trouble from the smoke and soot from these very boilers; I live near there. Now the air is comparatively clear and very free from soot.

MR. HUNT: Have they added to the number of the boilers?

MR. HERRON: I do not know how many they have, but they have just put in new boilers. Some twenty-five or thirty years ago I was in Erie. They were then just beginning to use Pittsburg coal, and the people complained very much about it. Erie was a clean place. They put steam jets under the boilers. I went to see it. It did away with the smoke entirely. They put the jets over the fire under the boilers. They said it was a perfect success. I do not see why that could not be used here under small boilers. It seems to me that would be a very economical way of reducing the smoke.

MR. R. L. WALKER: In regard to what Mr. Herron has said about the 13th ward pumping-station, tubular boilers were recently put in, the Roney stoker added. The steam jet was abolished. The addition of two boilers makes the 33 per cent. saving.

MR. HUNT: That shows the same line of statement we have given out in the report of the committee, that by proper appliances the smoke can be prevented, and then, again, it shows that two boilers have been added, giving a sufficient steam capacity to do the work under reasonable firing without crowding it. Mr. Herron makes the remark about a steam jet doing the work, and cites a case where it worked successfully. In some cases it undoubtedly will, probably by the action of steam as an injector, bringing in a sufficient amount of air to give a proper mixture of the oxygen of the air to burn up the carbon. But if the boiler is crowded beyond its capacity, and the arrangement and appliances of the boiler are, under these circumstances, of a character not sufficient to burn the products of combustion freely, as one of the gentlemen has just said, the jet will do no good. It requires correcting appliances back of that, and, as before stated, adding to the quantity of the steam-producer has been one of the principal causes of the prevention of smoke.

I would like to add a statement from my experience in regard to the value of the Roney stokers: they give very satisfactory results. In a plant of which I have charge there are three of them that are doing excellent work; still, when we try to run the whole plant with one boiler in the place of three, as we do sometimes, owing to accidents or other causes, we made smoke with the Roney stoker the same as if it were not there.

MR. MCGEARY: The greatest trouble about rolling-mills is the insufficient number of boilers. But in a fairly constructed furnace that is built to do certain work, and not pushed too much, it is not necessary to produce a great amount of smoke.

MR. WM. WHITE, JR.: The committee state that "producer-gas is not economical under boilers." I would like to have some facts and figures for this statement for the reason that Dr. Siemens' experiments showed that there was a gain of 25 per cent. by converting the coal into gas and burning the gas. Are the committee prepared to say that the heat-units are lost in the producer itself, or is the loss due to the fact that the heat is not absorbed on account of faulty construction of the boiler for gas-fuel? They also state that smoke is necessary in puddling. The National Tube Works Company has been puddling in Siemens' furnaces for twelve years, and produce no smoke. The whole matter appears to be like this: If an establishment will put in the modern improvements, no annoyance will be felt by the public. The trouble is with the old works.

MR. SCAIFE: Mr. Metcalf has paid especial attention to this subject, and will doubtless be pleased to give reasons for the non-success of producer-gas under steam boilers. The report recognizes the possibility of reducing smoke to a minimum with regenerative furnaces and natural gas, but in view of the fact that both puddling and natural gas are fast disappearing here, it is not likely that our iron manufacturers will consent to change their present furnaces to the costly regenerative type, or attempt to use natural gas again for puddling, especially as fair results can be obtained by other means at reasonable expense.

Mr. Wm. Metcalf: I know of Dr. Siemens' experiments and also of some very elaborate and exhaustive experiments by Dutch engineers carried out at great expense. The Crescent Steel Works spent some thousands in following out the same lines. All of these efforts failed.

The common sense of the matter is very simple. The heat consumed in the producer in making CO is lost for steam purposes; the burning of CO without regeneration does not produce intense heat. Regeneration under a boiler is practically impossible because the very object is to have the boiler absorb all of the heat of the fire. The only regeneration possible is to be obtained from the lower side of the flame as it sweeps along the boiler furnace; the amount of heat to be obtained in this way is not nearly enough to compensate for the heat lost in the producer. This is the whole story; if any person has money to expend in further experiments there can be no objection to the effort.

A Member: I remember seeing in the London *Engineering* a statement as to the use of producer-gas under boilers. I cannot state just when it appeared but the result was it was not economical.

Mr. Brashear: I have the pleasure of being associated, with some other of my friends here, with Prof. Langley in a long series of investigations as to the heat of the sun as applied to actual work. A great many investigations were made, and the fact that it was a failure was simply because of our lack of knowledge about utilizing the heat, and I think the assertion as to the non-success of producer-gas under boilers can be explained from the fact that they have not yet found the proper way to utilize it. The heat-units are there if we but knew how to apply them. I believe, however, that if the fuel of the whole of the coal-fields of Pennsylvania was shovelled into the sun it would keep up the temperature just one second; I am sure that is exact. I believe that the time will come when the engineers of this or some other country will take the original source of energy, the sun itself, and utilize its energy. It is the source of the fuel we are using at the present day. Our coal is simply the stored up energy of the sun of past ages, as is also our natural gas. The time will come when engineers will take the energy of the sun itself and utilize it. The experiments already made by Ericsson and Mouche, Langley and others give us great encouragement in this valuable line of research.

Adjourned.

R. N. CLARK,
Secretary.

NOVEMBER 22D, 1892.

MINUTES OF CHEMICAL SECTION MEETING.

Prof. F. C. Phillips, Chairman, and Mr. James O. Handy, Secretary. Nineteen members were present.

Prof. Phillips appointed the following gentlemen as members of the Committee on Chemical Literature: Mr. James O. Handy, Mr. James Camp, Mr. R. B. Carnahan, Jr., Mr. A. McKenna and Mr. Fred Crabtree.

The Chairman spoke of the proposed meeting of the American Chemical Society in Pittsburg, December 28th and 29th. Mr. James Camp moved: That the Secretary be instructed to offer the American Chemical Society the use of the rooms of the Engineers' Society for their Pittsburg Meeting. This motion was put and unanimously carried.

Mr. S. A. Ford exhibited and described his multiple filtering apparatus. This machine had been previously described (*J. A. A. C.*, Vol. VI., No. 9), but had not before been publicly exhibited by its inventor.

Mr. Weed, of the Western University of Pennsylvania, described a satisfactory way of coating labels for use in chemical laboratories, so that they would withstand the action of moisture and acid fumes.

The Secretary read a "Note on the Use of Hot Acid in the Evolution of Sulphur as H_2S from Pig-Iron," by Frederick H. Williams, of the Riverside Iron Works, Wheeling, W. Va.

The various subjects brought up were freely discussed.

DISCUSSION FOLLOWING MR. S. A. FORD'S DESCRIPTION OF THE MULTIPLE FILTERING APPARATUS.

MR. HANDY: Do you ordinarily use lip beakers?

MR. FORD: It does not make any difference. I generally order lipped beakers, but these that I have here to-night are without lips. None of the solution runs down the side of the beaker. For hand work, I know the lipped beakers are nicer.

MR. HANDY: Can the gas burners be regulated so that all will give the same heat?

MR. FORD: The gas supply can be regulated very nicely by means of the switches on the apparatus.

MR. A. EASTWICK: What kind of gas burners do you use?

MR. FORD: The ordinary Argand burners. They make a nicer heat than the Bunsen. The Bunsen burner gives heat only at the centre; the Argand gives heat all around. It can be turned down to a blue flame, and it will never go out, or it can be turned up to a boiling heat. Speaking of the regulation of the different burners, I have one of the machines with 12 burners, all regulated by one valve.

MR. EASTWICK: The nearest one gets the most gas.

MR. FORD: Yes; but the burners can be regulated so as to obviate this fault. I have the apparatus so arranged that two connections can be made—one on each side.

MR. EASTWICK: Does not the apparatus shake?

MR. FORD: Not when it is screwed down tightly.

MR. HANDY: Of what material is the plate on which the beakers stand?

MR. FORD: Cast-iron. The plate is movable to allow for contraction and expansion. If you happen to break it, another can be put in without any trouble. The only wooden parts about the apparatus are the top cross-pieces, which are made of wood so that there will be no danger of dropping iron into the solutions.

MR. EASTWICK: How much stirring do you say it requires?

MR. FORD: I said five minutes, but three minutes will do. Of course, when there is a small amount of phosphorus, it takes longer stirring than when there is a large amount. Temperature also affects the length of time necessary to stir. I heat the solutions up to

85° C., then put in 80 cubic centimetres of molybdic acid, so that the precipitation is made at 55° C.; but if the solution is allowed to cool to 50° C., and then put in the molybdic acid, it will come down in about eight minutes. Steel having .01 per cent. or .02 per cent. phosphorus is the lowest I ever worked with this apparatus, using at that time 1.63 grammes of the sample.

Mr. Eastwick: Do you find that the precipitate is granular when formed at lower temperatures?

Mr. Ford: I did not find any difference. It is only the rapid stirring that gives the granular precipitate.

Mr. Eastwick: Do you find that any of the precipitate climbs over the side of the filter-paper—any creeping?

Mr. Ford: No, there is no creeping. The washed Swedish filter-paper is very efficient. When I started in the analytical profession we used an old German filter-paper that took about half an hour to filter a pint of water. Solutions run through the washed Swedish filter-paper in a stream.

Mr. Eastwick: What wash solution do you use for the phosphorus precipitate?

Mr. Ford: I use a one per cent. nitric acid solution, and when using the alkali method, use finally nitrate of potash solution,—a very weak solution.

Mr. Eastwick: In the case of reducing the phospho-molybdate with zinc, do you not find it necessary to wash out the one per cent. nitric acid?

Mr. Ford: No; there is very little there. I never had any trouble. It takes longer for the reduction method than for the alkali method. I never favored weighing the yellow precipitate. There are two filters to weigh. The filter must be dried and weighed, and the precipitate also. With the alkali method there is no trouble of that kind at all. Dr. Dudley recommends using a little "reducer," but I never had much success with it; it took too much washing.

Mr. Handy: Speaking of the zinc reduction method for phosphorus determinations, it seems to me there is always one objection to it, namely: a correction must be made for the zinc. This makes a little complication.

Mr. Ford: In working this method, I always use a standard iron in which the phosphorus has been determined gravimetrically.

The Chairman: I should think one great advantage of the apparatus is the absolute uniformity of conditions.

Mr. Ford: So it is. I do not know how a more uniform heat could be obtained. With a hot plate there are hot places and cool places, but with this apparatus, the temperature can be regulated exactly as you want it.

Mr. Eastwick: Are you in the habit of finishing the determinations by titrating with an alkali solution?

Mr. Ford: I found that the alkali was a great deal quicker than any other method, and gave reasonably correct results. These determinations were for the purpose of knowing how the furnace was working. They agree within .002 per cent.

The Chairman: At the University of Western Pennsylvania we have been making a study of methods of mounting small specimens of minerals and material of various kinds that it is desirable to exhibit, in such a way as to protect them from dust, and I thought it might possibly be of interest to show you some of the specimens. Mr. Weed has been very successful in the work he has done in mounting and fixing such specimens, and I will ask him to say a few words on this subject.

Mr. Henry T. Weed: There seems to be two great troubles in mounting specimens. First, the labels almost always turn yellow after some time; and, second, the acid fumes of the laboratory corrode them. We find that it is possible to make a label which will last. The labels on the specimens exhibited here to-night have been exposed for over a year to the direct action of the sunlight, and to the acid fumes in the laboratory, and yet they do not show any signs of darkening, and are not corroded in the least. We found that the best way of preserving the labels was, first, to apply a very thin coat of gelatine. Take ordinary gelatine, such as is used for photographic purposes, and make a solution in water, just strong enough to gelatinize. Put a coat of that on the label, and let it dry thoroughly; then put on a weak solution of some transparent gum, such as gum sandarac dissolved in alcohol. The solution dries almost instantly of course. That label does very well when you are not very particular about permanency; but if a thoroughly permanent label is wanted, put on a coat of "Enameline," which is a patent varnish that can be obtained, I think, at the varnish stores. That makes a glossy label, and one which, as far as our experience goes, seems to be absolutely impervious to acid fumes. Strong alkali destroys it, but gum sandarac does not seem to be attacked by alkali. If you should chance to get strong alkali on the label, wash it off, allow it to dry, and put on another coat of "Enameline." In mounting small specimens, we find the blocks shown here to-night very useful. We take ordinary beeswax; put a little burnt umber, or any similar coloring matter, in it, melting in thoroughly. Then soften the wax by heating, and it sticks then to the surface of the wood. Push the small crystal right into the wax, and we will find that the crystal adheres well, and that there is very little danger of its coming loose. Then if we just touch with the hot point of a file the points at which the wax touches the crystal, the crystal adheres so that there is practically no danger of its coming out. (A crystal mounted in the manner described was here passed around among the members.) This method of mounting is very convenient, shows the crystal off nicely, and takes very little time. It does not take over half an hour or three-quarters of an hour to prepare fifteen or twenty labels. For rough, off-hand labelling of bottles containing solutions, we found a simple method. Take some black asphaltum (which is sold very cheaply), thin it with turpentine until the solution has the consistency of very thin molasses, and apply it to the bottles by means of a fine brush. It makes a very black mark which lasts well. The labels on bottles which have been in use by the students at the laboratory for over a year, and been subjected to very hard usage, both with acids and alkalies, are in good condition to-day.

Mr. HANDY: The subject of preserving labels has always interested me. I have frequently heard of cases where the chemists in laboratories have been changed, and the new chemist has had to throw away a great many solutions and chemicals on account of the labels having partially or wholly dropped off. I myself have always used a solution of shellac and alcohol. It makes the label rather unsightly, as it soons turns brown, but it is permanent.

Mr. Weed: I have a label here which has been subjected practically to that treatment. It decidedly does make the label unsightly. A coat of glue or gelatine is first needed in order to prevent the varnish from sinking into the paper. (Two labels were here shown, one made by the method described by Mr. Weed, and on the other, shellac had been used). You will notice that there is no comparison between the two as to looks; the first is far superior, and, of course, it is just as permanent as the other, and the difference in the time required to prepare them is very slight.

The Chairman: The principal advantage of this method is, after all, in the labelling of specimens of minerals, etc., although, of course, the bottle label is very important too.

Mr. Handy: Will the solution of gelatine last for some time, or must it be made up at the time it is wanted for use?

Mr. Weed: You can add some antiseptic to keep it.

Mr. Handy: Do you weigh the gelatine, and measure the water? I think that information would possibly be useful.

Mr. Weed: You will find that the temperature of the laboratory has some effect, and it is not possible to make a solution of the same strength in summer that you use in winter.

Mr. Weed uses the following solutions:

(*a*) *Solution of Gum Sandarac.*—Gum Sandarac, 1 ounce; 95 per cent. Alcohol, 10 ounces.

(*b*) Enameline comes ready for use. Use a drop of nitrobenzol to preserve the gelatine solution.

NOTE ON THE USE OF HOT ACID IN THE EVOLUTION OF SULPHUR AS H_2S FROM PIG-IRON.

BY FREDERICK H. WILLIAMS, CHEMIST RIVERSIDE IRON-WORKS, WHEELING, W. VA.

In response to the invitation of our secretary to prepare a paper on some subject for presentation at this meeting, I will give the results of a few experiments made by me about a year and a half ago and subsequently, on the use of *hot* hydrochloric acid for the solution of the iron in the volumetric determination of sulphur in pig-iron by the iodine method.

While thinking what plan might be tried to effect a complete elimination of sulphur, so that none should be left in the black residues in the flask, I concluded to try adding the dilute hydrochloric acid at a boiling temperature to the drillings in the flask and applying heat under the flask at once. The gases were passed through a continuous delivery-tube to the soda solution (50 c.c.) in a large test-tube. The soda solution contained 40 grammes NaHO to the litre (1.04 sp. gr.). The acid was 1.10 sp. gr. Two grammes of iron were used. The operation was finished when the gases ceased and the delivery-tube was hot near the test-tube, about 5 inches from the flask. The following table shows the results obtained on various samples compared with those obtained with cold acid and slow evolution of gas in the same flask:

Boiling Acid. Per cent. Sulphur.	Cold Acid, Slow Evolution. Per cent. Sulphur.
.064	.051
.049	.039
.057	.040
.013	.008
.060	.050
.186	.159
.097	.087

More iodine was consumed in the one case than in the other. The percentages are given on the supposition that the increase was due to sulphur.

The results were invariably lower with cold acid, varying from .005 per cent. in the lowest to .027 in the highest.

Another step in advance of this procedure was taken by mounting the apparatus with the iron drillings in the flask, adding a measured quantity of water, *boiling* till air was expelled, then quickly removing the lamp and adding boiling hot hydrochloric acid of 1.20 sp. gr. in quantity equal to the water in the flask, and replacing the lamp with a low flame. The results by this plan were generally a little higher than by the preceding one, as for example:

Boiling Conc. Acid and Boiling Water. Per cent. Sulphur.	Boiling Dilute Acid. Per cent. Sulphur.	Cold Acid, Slow Evolution. Per cent. Sulphur.
.049 .050 .051	.044	.030 .034

The time of operation in the flask was about 5 minutes. The gases were evolved and absorbed in 3 or 4 minutes.

Several tests were made to ascertain if any H_2S escaped absorption by this violent and rapid evolution. A second test-tube, with 40 c.c. soda solution was attached. This quantity of reagent by separate tests was found, when acidified and starch added, to require 2 drops of iodine solution (equal to $\frac{1}{10}$ c.c.) to produce a faint blue coloration. Never more than 2½ drops were required to produce the same blue tint after the gas had passed through, thus showing that practically no H_2S escaped the first tube. A blank operation was also conducted by placing the hot acid in an empty flask and boiling as in a regular analysis. The soda solution required no extra iodine to produce the blue tint. A genuine blank analysis would require the presence in the flask of an iron containing the usual ingredients—carbon, phosphorus, etc., but *no sulphur;* a thing unattainable.

On examining the residues in the flask (the residues from several similar operations were combined) by the method given by Mr. Blair, viz., filtering the contents of the flasks, washing, drying, fusing with sodium carbonate and potassium nitrate, etc., the following observations were made: *First*, there was a marked decrease of sulphur in the residues by this quick treatment with hot acid, but never an entire removal of it. *Second*, the decrease of sulphur did not correspond with the increase apparently evolved by the hot acid; in other words, the gain was greater than could be accounted for by the decrease in the residues. Not enough tests were made to warrant drawing conclusions on this point, however. *Third*, the best results of this rapid method were still below those yielded by the aqua regia method, with fusion of the first precipitate of $BaSO_4$ with sodium carbonate.

On treating *steel* with hot and cold acid, I found a greater uniformity of results with hot acid. With cold dilute acid there was often considerable variation.

These investigations suggest a few questions:

Is the increased amount of iodine solution consumed when using hot acid due to sulphur? If so, is the sulphur derived wholly or in part from what would have been retained in the residues by slow treatment with cold acid? If only in part from this source, whence comes the rest of the increase? Is sulphur sometimes liberated in such a form as not to be absorbed by NaHO? Could the varying results on steel be accounted for in this way? Is any other absorbent superior to NaHO?

In the practical application of this matter of rapid evolution I have been very largely anticipated by Mr. Troilius, who published, in *Transactions* American Institute of Mining Engineers, vol. xii., p. 507, his method of determining sulphur in steel by the bromine method. In this method he added 100 c.c. boiling water to the flask, and then 100 c.c. strong hydrochloric acid, cold. I do not know how far some of the members of this society may also have gone in investigating the subject, but it seemed to me that it might prove interesting to have the subject discussed.

DISCUSSION.

MR. HANDY: Mr. Williams's paper is a very interesting one, and ought to excite a good deal of comment and further investigation on our part. I myself have always used the cold acid with final boiling up to effect solution.

MR. WEED: I think he gives two sets of figures—.049 per cent. and .030 per cent.—and then says his best results are considerably below the result obtained by aqua regia. The difference between .049 per cent. and .030 per cent. is practically 40 per cent., and even the .049 per cent. is below the aqua regia method. That would make probably 45 or 50 per cent. of error in the evolution method. Is that not outside of the limits of error?

MR. FORD: I have had some little experience in sulphur determinations, and I came to the conclusion that some of the sulphur in iron exists as an organic compound. We know that it exists in coal as an organic compound. In 1875, I was with the Geological Survey, and we made a series of experiments on sulphur in coal, and obtained a very curious lot of results. We would get a coal with a certain amount of sulphur, in it, a certain amount of iron, and a certain amount of lime. Calculating all the iron and lime to sulphids and sulphates does not require all the sulphur found. Given two coals with the same amount of sulphur the one has a high percentage of sulphid of iron, and the other a low. It was not possible to foresee which would retain the more sulphur when made into coke. In the case of pig-iron, I came to the conclusion that the sulphur existed as a carbide, and that the evolution method did not give accurate results. I have not used the evolution method for years, except as a guide on our furnaces, but have always used the aqua-regia method. Aqua-regia is the name given to a mixture of acids that has one part of hydrochloric acid to 99 parts of nitric acid, or 99 parts of hydrochloric acid to one part of nitric acid. In the mixture I made, there was just enough hydrochloric acid to start the iron dissolving—1 c.c. of concentrated hydrochloric acid and 50 c.c. of the strongest nitric acid, which was put on a steam bath in which the temperature could not get over 300° C. When the iron is all dissolved, I put in about 100 c.c. of strong hydrochloric acid, and evaporate that down to dryness; take up the next morning with 100 c.c. of hydrochloric acid, and evaporate that down until the chlorides begin to crystallize, then dilute with water and filter; put in the barium chlorid, and let that stand over night. Now, if you want to be absolutely accurate, take that precipitate and fuse it with carbonate of soda, dissolve in water, filter, acidulate the filtrate with HCl, evaporate to dryness, redissolve in water and about three drops of acid, filter and add chloride of barium, allow to stand twelve hours, filter, ignite and weigh. If the temperature is too high in the aqua regia method, some of the sulphur will be driven off. The evolution method is satisfactory for sulphur determinations in steel, but not always in pig-iron.

MR. HANDY: In the analysis of pyrites for sulphur, Dr. Lunge dissolves the material in aqua regia, evaporates to a small bulk, and gets finally a hydrochloric solution, which he dilutes, precipitates the iron by ammonia, heats almost to boiling and filters. He acidifies the filtrate and precipitates with barium chloride. Possibly that may be a good modification to work into the aqua regia method for sulphur. The $BaSO_4$ being precipitated in a solution which is free from iron, will need no fusion to purify it.

Mr. Ford: The fusion was merely done in checking experiments to see whether there was much difference. Not once in a hundred times do I make a fusion, because it takes twenty-four hours longer.

Mr. Handy: What amount of iron do you work with?

Mr. Ford: Five grammes. I would like to talk about a little experience I had. Dr. Dudley, in speaking about the discrepancies in chemical analyses, mentioned the very different amounts of sulphur found in a piece of pig-iron. I came across one cast of pig-iron from a furnace from which I took seven samples, and the sulphur in them varied from .015 per cent. to .150 per cent. Now, I defy any two chemists to take two samples out of that iron and get the same results. And the silicon varied from .5 to 5 per cent. The furnace was working very badly. I merely give these as examples. I have often wondered how two chemists get results so close together.

Mr. Eastwick: In the case of finished steel, would these results not be more uniform? Is not the sulphur more evenly distributed when the iron is made into steel? The discrepancy ought not to be so great.

Mr. Ford: This brings me to a subject in which I have a great deal of interest. In 1881 I made the first lot of experiments on that, and I found that sulphur segregates more than any other impurity. I found .030 per cent. and .072 per cent. in the same ingot, running from the bottom to the top. And the phosphorus—well, we all know how that runs.

Mr. Eastwick: That has been my experience also; that is why I asked that question.

Mr. Ford: No, sir; steel is not homogeneous; never is. Out of the same ingot the sulphur varied from .035 per cent. to .068

per cent. or .070 per cent. I do not want to take up your time, but, if not wearying you too much, I will mention an experiment I made. I found around a works an ingot which, for certain purposes, had been cut in two and made into a slab—the very thing I wanted. In that piece I drilled in the centre every three inches from the bottom to the top, and along the edge every three inches from the bottom to the top for samples. Starting at the bottom, the phosphorus was .087 per cent.; a foot from the bottom it decreased to .072 per cent. then it increased gradually, with perfect uniformity, to the top, where the phosphorus was .165 per cent. On the outside edge it was lower, but it gradually increased from the bottom to the top. One foot from the bottom the phosphorus was exactly in the same proportion as that taken from the centre. I took a rail, cut it down, and etching it with dilute nitric acid, found a difference of from .070 per cent., I think, in the outer edge, to .350 per cent. in the centre; and yet, in some rails, I could not find any difference at all. In my experience, sulphur has always varied the most.

Mr. Weed: I would like to ask Mr. Ford whether he would not consider the getting of .030 per cent. instead of .050 per cent. an exceptionally poor result for the evolution method?

Mr. Ford: I never thought very favorably of the evolution method. Some years ago a chemist in England made experiments on this, and the evolution of sulphur varied from 25 to 75 per cent. There was from 25 to 75 per cent. of the sulphur remained insoluble, but that is not always so.

Mr. Camp: I feel rather partial to the evolution method. We have to make about a hundred determinations a day, which could not be done by any other method, and we are pretty nearly ready to swear to the accuracy of our determinations. They are close enough to run a steel works on. I have had cases where we got .03 per cent. by the evolution method and .06 per cent. by the aqua regia method, but it seems to me these discrepancies are exceptional cases, arising from a peculiar condition of the pig-iron or the working of the furnace, and are the exception, not the rule. We cannot afford to throw away the method on that account, and I purpose to stick to it. I believe 99 per cent. of the determinations by the evolution method are within the limits of error—.005 per cent.

MR. HANDY: I think the point brought up by Mr. Williams is worth investigating, and I shall do some work on it before the next meeting, and shall have an opportunity to try whether the absorbent makes any difference in the result. I myself use a cadmium salt, and I shall try several experiments on pig-iron and steel.

Society adjourned at 10 P.M.

JAMES O. HANDY,
Secretary.

DECEMBER 20TH, 1892.

THE Society met in the parlors of the Academy of Science and Art, on December 20, 1892.

Mr. Alfred E. Hunt, President, in the chair; fifty-two members were present. The minutes of the last meeting were read and approved. The following were elected members, the Secretary casting the ballot: John L. Klindworth, G. P. Van Marken, Jesse A. Dubbs, Harry Menough, W. R. Macdonald, William H. Balzell, C. G. McKee, William White, Jr., Stansbury Sutton, F. duP. Thomson, P. T. Berg, John C. Neale, Ph. Vierheller.

At the request of the Ladies' Health Protective Association, Mr. W. Lucien Scaife read a letter from the Gregory Furnace Company, describing the claims of the owners of the furnace to decrease smoke.

The special business for this meeting being in order, the question of amending Article III. of the By-Laws was called.

Mr. George S. Davison took the floor. He called attention to the tabulated vote, written on the blackboard, which expressed the opinion of those of the members who had forwarded return postal cards to the Society as had been requested.

The vote expressed the opinion of 190 members in regard to how much should be charged for annual dues; it was as follows: 23 voted for $5 per annum, 36 voted for $6, 75 voted for $7, 54 voted for $8, 2 voted for $10.

After a very full discussion, on motion of Mr. Thomas H. Johnson, it was carried by a vote of 44 ayes to 1 no, that the initiation fee shall be $5; the annual dues for members residing in Allegheny County, Penna., $7 per annum; for members residing elsewhere, $5 per annum.

It was ordered that the committee on annual dues, consisting of Messrs. Thomas H. Johnson and G. S. Davison shall prepare a corrected copy of the amended By-Law, and that the Secretary should have printed copies of the Constitution and By-Laws as now amended, together with the Rules of the Chemical Section.

In the absence of the author, Mr. W. Lucien Scaife read the paper announced for the evening.

THE CONSTRUCTION OF IRON AND STEEL WATER TANKS.

BY WILLIAM C. COFFIN, JR.

Iron and steel tanks are in quite general use in public and private water works as storage reservoirs, settling tanks and standpipes. Generally they are designed to act, to a certain extent, in each of these capacities.

The question of dimensions, both of cubic contents and of proportion of height to diameter, is one that must be based upon, and decided by, the special local requirements, and need not be entered upon in any detail.

The best economic proportions for storage tanks is to make the height from two-thirds of the diameter to once the diameter.

The height of settling tanks should be about one-fourth of the diameter.

Standpipes made of the height to give the required head or pressure should have their diameters not less than one-tenth of the height.

The foundations of standpipes should receive careful attention and the base proportioned to suit the nature of the soil and also to make sufficient anchorage.

Storage tanks, however, need only a ring wall with gravel or concrete packing under the bottom, and when the height is small the ring wall may be abandoned and the tank set on a levelled grade, with all vegetable soil removed, well trenched and drained outside to avoid storms from washing the ground from the outer bottom. If the surface is of a clayey nature, the trench should be lined with cement as a further precaution.

The inlet and outlet pipes should be underground and below the frost line, and enter the tank in the bottom using an elbow with a foot or bearing and a vertical length of pipe passing through a gland which is riveted to the bottom. This connection should be in, at least, 3 feet from the shell.

Connections made on the shell, or which are riveted direct to the bottom, are subject to heavy temperature strains which endanger the safety of the joint.

Anchor bolts, when needed at all, should always be proportioned to insure the stability of the tank under the heaviest wind pressures without allowing for any weight of water. The connection to the shell should be designed to bring the rivets into direct shear, and proper re-enforcement of the shell should not be neglected.

Where the proportion of height to diameter is great, brackets may be economically employed to increase the width of the base. They are often used, however, when not necessary, as they add somewhat to the appearance, but unnecessary connections are not only extravagant, but add greatly to the danger of leakage.

Tanks and standpipes should be made of good commercial iron or steel and should be critically inspected for defects of any nature, but high-grade boiler or flange qualities are quite unnecessary excepting in cases of extremely large tanks where it is desired to use every means of decreasing the thickness of the plates.

It is needless to say that steel is now used almost entirely owing to its low cost and superior strength. A fair specification is: "From 57,000 to 65,000 pounds tensile strength, with an elongation of 20 per cent. in 8 inches."

While the best practice is to drill all rivet holes, or to punch them $\frac{1}{16}$ inch smaller than the rivet, and then ream them out $\frac{1}{8}$ inch larger, it is not customary, nor in the writer's opinion necessary, except in very heavy work.

In thickness, nothing less than $\frac{3}{16}$-inch plate should ever be used, while ¼-inch thickness is the best minimum to establish.

In large tanks the bottom plates should be of ¼ inch or $\frac{5}{16}$ inch. In standpipes ⅜ inch or ½ inch should be used. The plates in large tank bottoms can be lapped at the joints, but inside butt straps make the best work for standpipe bottoms. The rivets should not be countersunk on the bottom as is sometimes specified, as full heads make stronger and tighter work.

The bottom should be lowered on a fresh cement grouting on top of the foundation which insures a uniform bearing.

The proper factor of safety of the shell is a question which has been largely discussed by those interested, and anywhere from two to ten is used by different engineers.

The writer uses, as a minimum, a factor of three, with ⅛ of an inch added for corrosion, based on the strength at the joint.

The strength of joints and the spacing or rivets are subjects upon which volumes have been written. Very safe close values, when rivets are properly spaced, for strength of joints are:

For single riveted joint, 50 per cent. of plate.

For double riveted joint, 65 per cent. of plate.

For triple riveted joint, 80 per cent. of plate.

Common practice is to double rivet all vertical seams and single rivet the horizontal ones unless the diameter is so small and height so great that the wind strains require the lower rings to be double riveted on the roundabout joints.

The best proportion of rivets is to use a diameter equal to double the thickness of the plate, thus: for ¼-inch plates use ½-inch rivets, for ⅜-inch plate, ¾-inch rivets, but 1-inch rivets can be used to ⅝-inch thickness and 1⅛-inch rivets to 1-inch thickness, not that it is best in theory but because heavy rivets cannot be used to advantage in erection, and it is best to triple rivet a joint with 1⅛-inch rivets, than to double rivet it with 1⅜ inch. The best spacing is to pitch the rivets three diameters from centre to centre along a single joint, thus:

Half-inch rivets spaced 1½-inch centres.

One-inch rivets spaced 3-inch centres.

In double and triple joints use four diameters for the pitch on each line, but draw the lines close enough to have the rivets pitched three diameters diagonally.

The net distance from the centre of the rivet line to the edge of the plate, after being beveled for caulking, should be one and one-half diameters of the rivet, plus $\frac{1}{8}$ of an inch.

Plates should be about 5 feet wide and from 9 to 13 feet long. There are practical objections to excessively large plates that overbalance the advantage of the saving of joints. They are more subject to warping in manufacture and get out of shape in transporting.

Alternate inside and outside rings are to be preferred to "telescoping."

The outside edges of all sheets should be bevel sheared or planed to about 60° for caulking with a blunt tool.

Inside caulking should be called for on thicknesses over $\frac{5}{8}$ inch, but in lighter gauges it is apt to undo the outside work and is unnecessary.

The bottom should be connected to the shell in all ordinary cases by an internal angle iron, as this allows the shell and bottom to be caulked against the angle. When it is desired to give the shell a greater bearing on the bottom (in large standpipes) both internal and external angles can be used, the outer one for strength, and the inner one of light thickness for caulking to the shell and bottom.

Specifications often call for the bottom to be flanged for the connection to the shell, or for the bottom to be of cast-iron; or of stone or concrete with the shell imbedded. It is mild criticism to say that these methods are imperfect.

A large manhead should be provided in the lower ring for use in constructing, cleaning and repairing. The best form is a circular wrought-iron flanged neck 20-inch diameter with a bolted cover plate of the same material.

The elastic flange can be well caulked.

Cast-iron manheads are heavy and treacherous.

Spiral stairs and galleries are often required. In their design a few substantial brackets for supports are much better than direct connections. Generally only a ladder is needed, $\frac{3}{4}$-inch rounds

12-inch centre, with 2- by ½-inch side rails 18 inches apart fastened every 10 feet to the tank will make substantial work. The ladder should end 10 feet above the foundation.

One 3- by 5-inch angle around the top will provide against the collapsing of an empty tank, from wind pressure, to a diameter of 20 feet.

Two angles will do a diameter of 30 feet.

Larger tanks should have cross bracing in addition to the angles. The simplest form is a series of rods connecting to a ring in the centre, in the form of a bicycle wheel.

When the top is properly braced, lower bracing is unnecessary, and intermediate rings of angles or tee-iron often specified should be omitted as they are a great source for leaks.

Proportions for Riveted Joints.

Thickn'ss of Plate.	Diam. of Rivet.	Single Riveting Centres.	DOUBLE RIVETING.		TOTAL LAP.		
			Centres On Line.	Space Between Lines.	Single.	Double.	Triple.
3/16	3/8	1 1/8	1 1/2	3/4	1 3/8	2 1/4	......
1/4	1/2	1 1/2	2	1	1 3/4	2 3/4	......
5/16	5/8	2	2 1/2	1 1/4	2 1/8	3 3/8	4 5/8
3/8	3/4	2 1/4	3	1 1/2	2 1/2	4	5 1/2
7/16 1/2	7/8	2 1/2	3 1/2	1 3/4	2 7/8	4 5/8	6 3/8
9/16 11/16	1	3	4	2	3 1/4	5 1/4	7 1/4
3/4 7/8	1 1/8	3 1/4	4 1/2	2 1/4	3 1/2	5 3/4	8
1'' 1 1/8	1 1/4	3 3/4	5	2 1/2	4	6 1/2	9

Dipping the plates in a hot bath of asphaltum before shipping, protects the material better than any other method of painting, but this should not be done unless the edges are afterward to be scraped so that the joints will have a close bearing.

With proper precaution during the manufacture and transportation the tank can be erected before painting, by scraping off rust with a steel brush, or the material can be given a single coat of linseed oil, any heavy or gummy joints being wiped off.

After erection three coats of paint are required for proper protection, and tests should be made beforehand to see that the mixture is of the proper temper, so as not to scale off. This can be done by dipping a coated piece of iron in cold water and also in hot and testing it with a hammer.

Asphaltum, oxide of iron, and lead paints give almost equal satisfaction; the lead mixtures are probably the most protective and durable.

The paper was fully discussed by several of the members.

Adjourned.

R. N. CLARK,
Secretary.

MEETING OF CHEMICAL SECTION.

DECEMBER 29TH, 1892.

Three joint meetings of the American Chemical Society and the Chemical Section of the Engineers' Society of Western Pennsylvania, were held December 28 and 29, 1892, at Pittsburg, and the following papers were presented: "The Effect of Decomposing Organic Matter on the Solubility of Natural Phosphates," Prof. N. B. Lupton, Alabama; "Official Milk Analysis," Prof. W. H. Ellis, Toronto, Canada; "Plant Lice Honey," Dr. H. W. Wiley, Washington; "Composition of Modern Powders," Prof. C. W. Monroe, Newport; "Lithographic Limestones," Prof. C. W. Volney, New York.

Prof. G. C. Caldwell delivered a retiring address on "The Work of the American Chemist." Plans for the Congress of Chemists at the World's Columbian Exposition were discussed, and excursions were made to various metallurgical works and factories.

JAMES O. HANDY,
Secretary.

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Dec., '90.	Abbott, W. L.,	Bank of Commerce Bldg., Pittsburg, Pa.
May, '80.	Aiken, Henry,	508 Lewis Bldg., Pittsburg, Pa.
March, '88.	Aikman, Edw. G.,	115 Broadway (Room 95), New York, N. Y.
Oct., '85.	Albree, C. B.,	18 Market St., Allegheny, Pa.
April, '80.	Amsler, Carl,	Bissel Block, Pittsburg, Pa.
Dec., '84.	Anderson, J. W.,	45 Fremont St., Allegheny, Pa.
March, '92.	Andrews, Ferdin'd,	Mansfield Valley, Pa.
Jan., '80.	Armstrong, H. W.,	Verona, Pa.
Jan., '87.	Arms, Walter F.,	Helvetia, Clearfield Co., Pa.
Nov., '88.	Arras, J. W.,	P. O. Box 485, Pittsburg, Pa.
March, '92.	Arrott, J. W., Jr.,	Standard Mfg. Co., River Ave., Allegheny, Pa.
April, '90.	Ashworth, Daniel,	108 4th Ave., Pittsburg, Pa.
April, '91.	Atwood, S. A .	Beaver Falls, Pa.
Feb., '82.	Aull, William F.,	66 3d Ave., Pittsburg, Pa.
Sept., '87.	Bailey, Chas.,	36th St. and A. V. R. R., Pittsburg, Pa.
Sept., '84.	Bailey, Jas. M.,	Sligo Iron Works, Pittsburg, Pa.
June, '92.	Bailey, Jno. M.,	116 Water St., Pittsburg, Pa.
May, '84.	Bakewell, Thos. W.	Bakewell Bldg., Pittsburg, Pa.
June, '88.	Bakewell, Wm.,	110 Diamond St., Pittsburg, Pa.
Feb., '92.	Ballard, Walter A.,	Lewis Block, Pittsburg, Pa.
Dec., '92.	Balzell, Wm. H.,	311 Lewis Blk., Pittsburg, Pa.
April, '88.	Barbour, Geo. H.,	Barbour, Gilfillan & Taylor, Chicago, Ill.

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May, '85.	Barnes, Phineas,	309 Shady Lane, Pittsburg, Pa.
May, '90.	Barret, J. H.,	184 Jackson St., Allegheny, Pa.
April, '92.	Barrett, J. C.,	Duquesne, Pa.
April, '92.	Barrows, W. A.,	Sharpsville, Pa.
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Nov., '92.	Beazell, Edwin H.,	Keystone Bridge Wks., Pittsburg, Pa.
Jan., '80.	Becker, Max J.,	5525 Ellsworth Ave., E.E., Pittsburg, Pa.
Jan., '85.	Beckfield, Chas.,	804 Duquesne Way, Pittsburg, Pa.
Nov., '89.	Bell, W. J.,	P. O. Box 976, Pittsburg, Pa.
Dec., '83.	Benney, Jas.,	Emsworth, Pa.
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Jan., '80.	Bigelow, E. M.,	Pittsburg, Pa.
Feb., '89.	Billen, C. E.,	Fraser & Chalmers, 12th and Rockwell Sts., Chicago, Ill.
March, '91.	Black, S. W.,	99 4th Ave., Pittsburg, Pa.
Sept., '83.	Blank, Hugo,	77 4th Ave., Pittsburg, Pa.
Jan., '89.	Blaxter, G. H.,	Allegheny Co. Light Co., Pittsburg, Pa.
March, '84.	Bole, Wm. A.,	25th St. and Liberty Ave., Pittsburg, Pa.
April, '81.	Boyd, Henry A.,	National Tube Works, McKeesport, Pa.
April, '92.	Brakes, Jas.,	3340 6th Ave., Troy, N. Y.
March, '84.	Brashear, John A.,	Observatory Ave., Allegheny, Pa.
Feb., '91.	Branne, J.,	Mt. Vernon Bridge Co., Mt. Vernon, O.
April, '87.	Breen, H.,	Denver, Col.

Date of Membership.			
Jan.,	'80.	Brendlinger, P. F.,	79 Warburton Ave., Yonkers, N. Y.
Jan.,	'86.	Brockett, Alonzo H.	Mellor & Hoene, 5th Ave., Pittsburg, Pa.
Jan.,	'80.	Browne, Geo. H.,	Westinghouse Bldg., Pittsburg, Pa.
Jan.,	'80.	Brown, W. R.,	City Engineer's Office, Pittsburg, Pa.
April,	'82.	Brunot, H. J.,	Greensburg, Pa.
March,	'92.	Brynn, P.,	Shiffler Bridge Co., Pittsburg, Pa.
Jan.,	'87.	Buente, C. F.,	Duquesne Way and 10th St., Pittsburg, Pa.
Jan.,	'80.	Bullock, W. S.,	Taylor & Bullock, Pittsburg, Pa.
Sept.,	'80.	Burger, Rutherford	Kidd Steel Wire Co., Ltd., Sharpsburg, Pa.
Jan.,	'86.	Cadman, A. W.,	63 Water St., Pittsburg, Pa.
May,	'82.	Camp, Jas. M.,	Duquesne, Pa.
Dec.,	'87.	Campbell, Hugh C.,	29 North Ave., Allegheny, Pa.
June,	'92.	Card, W. W.,	Penn and Murtland Aves., Pittsburg, Pa.
Feb.,	'83.	Carhart, Daniel,	Western University, Allegheny, Pa.
May,	'85.	Carlin, Thos. H.,	181 Robinson St., Allegheny, Pa.
Nov.,	'84.	Carlin, David,	West Market St., Allegheny, Pa.
Dec.,	'90.	Carnahan, R. B., Jr.	McKeesport, Pa.
April,	'90.	Carnegie, Andrew,	55 Broadway, New York, N. Y.
March,	'92.	Castrique, Chas.,	116 Water St., Pittsburg, Pa.
Sept.,	'83.	Chambers, J. S., Jr.,	19 Church Ave., Allegheny, Pa.
Feb.,	'80.	Chess, H. B.,	531 Wood St., Pittsburg, Pa.
Nov.,	'82.	Clapp, Geo. H.,	116 Water St., Pittsburg, Pa.

DATE OF MEMBERSHIP.		
Jan., '88.	Clark, R. N.,	32d and Smallman Sts., Pittsburg, Pa.
May, '89.	Clarke, Louis S.,	117 Water St., Pittsburg, Pa.
April, '92.	Clifton, Warren R.,	New Castle, Pa.
Sept., '91.	Clugston, W. A.,	531 Wood St., Pittsburg, Pa.
Oct., '83.	Coffin, Wm. C., Jr.,	66 Western Ave., Allegheny, Pa.
April, '87.	Colby, J. A.,	Girard Building, Philadelphia, Pa.
Feb., '91.	Comp, C. A.,	Westinghouse Elec. Co., Pittsburg, Pa.
May, '91.	Connelly, Cliff'd B.,	Western University, Allegheny, Pa.
Feb., '81.	Cooper, Chas. A.,	410 Grant St., Pittsburg, Pa.
Dec., '81.	Cooper, John W.,	Pitts. Locomotive Wks., Allegheny, Pa.
April, '92.	Cooper, Frank I.,	Taunton, Mass.
Oct., '92.	Corcoran, W. H.,	Howe, Brown & Co., Pittsburg, Pa.
Nov., '89.	Cornelius, W. A.,	B. & O. R. R., Hazelwood, Pa.
Oct., '92.	Coster, Maurice,	Westinghouse E. & M. Co., Pittsburg, Pa.
April, '92.	Coster, William H.,	Cor. Penn and 4th St., Pittsburg, Pa.
March, '92.	Crabtree, Fred.,	Monongahela Furnace Co., McKeesport, Pa.
March, '92.	Crobaugh, F. L.,	Sharon, Pa.
Sept., '80.	Curry, H. M.,	Carnegie Steel Co., Pittsburg, Pa.
April, '92.	Daly, W. H., M.D.,	6213 Howe St., Pittsburg, Pa.
April, '92.	Davis, Arthur W.,	116 Water St., Pittsburg, Pa.
Jan., '80.	Davis, Charles,	County Eng., Court House, Pittsburg, Pa.
Dec., '80.	Davison, Geo. S.,	Westinghouse Building, Pittsburg, Pa.

DATE OF MEMBERSHIP.		
March, '92.	Davison, Neville C.,	68 Water St., Pittsburg, Pa.
Feb., '91.	Deforth, John M.,	Keystone Bridge Works, Pittsburg, Pa.
Jan., '80.	Dempster, Alex.,	40 7th Ave., Pittsburg, Pa.
April, '92.	Dickson, Jos. H.,	Norristown Steel Works, Norristown, Pa.
Jan., '80.	Diescher, Samuel,	Hamilton Building, Pittsburg, Pa.
Nov., '87.	Dobson, Thos. H.,	Penn P. O., Lancaster Co., Pa.
Nov., '90.	Doxrud, Peter,	Park Bros, Pittsburg, Pa.
April, '84.	Dravo, H. C.,	413 Wood St., Pittsburg, Pa.
Oct., '90.	Dravo, E. T.,	49 Fifth Ave., Pittsburg, Pa.
Jan., '92.	Driver, C. M.,	P. O. Box 541, Pittsburg, Pa.
Dec., '92.	Dubbs, Jesse A.,	413 Wood St., Pittsburg, Pa.
March, '92.	Duckham, A. E.,	6216 Penn Av., Pittsburg, Pa.
April, '92.	Dudley, C. B., M.D.	Altoona, Pa.
March, '92.	Eastwick, I. H.,	130 S. Negley Ave., Pittsburg, Pa.
June, '92.	Eastwick, A. T.,	130 S. Negley Ave., Pittsburg, Pa.
Jan., '81.	Eckert, E. W.,	1313 Monadnock Bldg., Chicago, Ill.
Jan., '80.	Edeburn, W. A.,	410 Grant St., Pittsburg, Pa.
March, '91.	Edwards, J. P.,	Uniontown, Pa.
Jan., '80.	Ehlers, Chas.,	Allegheny, Pa.
April, '92.	Elliott, E. L.,	High School, Pittsburg, Pa.
Jan., '92.	Emmons, C. DeM.,	Dithridge and Forbes Sts., Pittsburg, Pa.
Feb., '88.	Engle, Geo. U.,	281 N. High St., Columbus, Ohio.
Sept., '82.	Engström, Franz,	Penna. Co., Pittsburg, Pa.
Feb., '88.	Estrada, E. D.,	307 Lewis Blk., Pittsburg, Pa.
Feb., '92.	Chas. J. Farrar,	128 Perrysville Ave., Allegheny, Pa.
April, '92.	Faunce, Geo.,	Mansfield Valley, Pa.

DATE OF MEMBERSHIP.		
March, '86.	Ferris, Geo. W. G.,	P. O. Box 539, Pittsburg, Pa.
Jan., '85.	Fitler, F. K.,	121 Water St., Pittsburg, Pa.
April, '89.	Fleming, H. S.,	Manuf. Record Bldg., Baltimore, Md.
March, '92.	Floyd, Jno.,	385 Wylie Av., Pittsburg, Pa.
Jan., '87.	Follansbee, Gilb'rt,	Supt. Chamber of Com., Pittsburg, Pa.
March, '92.	Ford, S. A.,	Kenmawr Hotel, E. E., Pittsburg, Pa.
April, '91.	Forter, S.,	Lewis Block, Room 307, Pittsburg, Pa.
Oct., '90.	Foster, Jas.,	Virginia Iron & Railway Co., Goshen, Va.
Feb., '82.	Frank, Isaac W.,	54th St. and A. V. R. R., Pittsburg, Pa.
May, '92.	Froenheiser, Jas J.	Cambria Iron Works, Johnstown, Pa.
Jan., '80.	Frost, A. E.,	Perrysville Ave., Allegheny, Pa.
April, '88.	Fulton, Louis B.,	34 Penn Ave., Pittsburg, Pa.
April, '90.	Giles, W. A.,	Lewis Blk., Pittsburg, Pa.
April, '92.	Gill, T. D.,	Sharon Steel Cast'g Co., Sharon, Pa.
Oct., '83.	Glafey, Fred'k A.,	Carnegie Steel Co., Pittsburg, Pa.
March, '92.	Glass, G. G.,	La Belle Steel Works, Allegheny, Pa.
Feb., '80.	Goodyear, S. W.,	Waterbury, Conn.
Jan., '80.	Gottlieb, A.,	Room 75, Major Blk., Chicago, Ill.
May, '92.	Graham, Neville B.,	Duquesne, Pa.
June, '85.	Grant, Horace E.,	119 First Ave., Pittsburg, Pa.
Nov., '92.	Gray, Robert,	New Castle, Pa.
Oct., '92.	Greenwood, G. F.,	Gen'l Mgr. P.&A.Traction Co, Allegheny, Pa.

DATE OF MEMBERSHIP.		
April, '85.	Griffen, A. L.,	Carnegie Steel Co., Pittsburg, Pa.
Jan., '92.	Griffen, Francis A.,	319 S. Highland Ave., Pittsburg, Pa.
Sept., '82.	Gwinner, Fred., Jr.	Allegheny, Pa.
March, '83.	Hackett, Geo. W.,	1009 Liberty St., Pittsburg, Pa.
Dec., '90.	Hall, Chas. M.,	116 Water St., Pittsburg, Pa.
March, '92.	Hall, J. G.,	Penna. Salt Works, Natrona, Pa.
Feb., '91.	Hallgren, Emil,	Duquesne, Pa.
Oct., '89.	Handy, J. O.,	116 Water St., Pittsburg, Pa.
Feb., '91.	Hardie, J. B.,	Keystone Bridge Works, Pittsburg, Pa.
April, '81.	Harlow, Geo. R.,	Wilkinsburg, Pa.
Jan., '80.	Harlow, Jas. H.,	Wilkinsburg, Pa.
Nov., '92.	Harkins, J.,	Millvale Ave., Pittsburg, Pa.
March, '92.	Harrison, A. B.,	7 LaBelle St., Pittsburg, Pa.
Nov., '91.	Hartrick, R. S. D.,	134 Water St., Pittsburg, Pa.
April, '92.	Harvey, John L.,	Park Bros. & Co., Pittsburg, Pa.
Nov., '92.	Hatch, Fred. T.,	Mansfield Valley, Pa.
Oct., '92.	Hays, Wm. H.,	32d and Smallman Sts., Pittsburg, Pa.
Jan., '80.	Hemphill, Jas.,	Mackintosh, Hemphill & Co., Pittsburg, Pa.
Nov., '85.	Heron, Fred.,	Supt. Phœnix Iron Wks., Phœnix, Pa.
Oct., '92.	Herron, W. A.,	80 4th Ave., Pittsburg, Pa.
Jan., '86.	Hetzel, Jas.,	60 4th Ave., Pittsburg, Pa.
April, '87.	Hibbard, H. D.,	High Bridge, N. J.
Nov., '88.	Hoag, I. V., Jr.,	U. S. Asst. Engineer, Frankfort, Ky.
April, '80.	Hoffstot, Frank N.,	Water St., Pittsburg, Pa.
Sept., '88.	Hohl, L. I.,	100 Boggs Av., Pittsburg, Pa.
May, '92.	Holbrook, E.,	Supt. B. & O. R. R., Pittsburg, Pa.

Date of Membership.			
Dec.,	'88.	Holland, W. J.,	Fifth Ave., Oakland, Pittsburg, Pa.
April,	'92.	Hope, Jas. A. P.,	E. Winter Co., Herr's Island, Allegheny Co., Pa.
Nov.,	'87.	Hopke, T. M.,	Linden Steel Company, Pittsburg, Pa.
March,	'92.	House, Frank E.,	P. & L. E. R. R., Beaver, Pa.
Oct.,	'88.	Howe, H. M.,	287 Marlborough St., Boston, Mass.
April,	'92.	Hull, E. B.,	Thomas Furnace Co., Niles, Ohio.
Oct.,	'81.	Hunt, A. E.,	116 Water St., Pittsburg, Pa.
Jan.,	'89.	Hunt, H. E.,	Emerson St., E. E., Pittsburg, Pa.
Oct.,	'90.	Hutchins'n, Geo.H.	Ft. Worth, Texas.
Oct.,	'87.	Hyde, C.,	Room 902, Lewis Block, Pittsburg, Pa.
Dec.,	'92.	Jackson, Walter H.	Schoen Mfg. Co., Allegheny, Pa.
March,	'80.	Jarboe, W. S.,	Room 69, Schmidt Bldg., Pittsburg, Pa.
Dec.,	'88.	Jenkins, J. B.,	Port Richmond, N. Y.
Feb.,	'81.	Jennings, B. F.,	Preble Ave., Allegheny, Pa.
Jan.,	'88.	Johnson, Thos. H.,	Penna. Lines, 10th & Penn Sts. Pittsburg, Pa.
May,	'92.	Johnson, J. P.,	116 Water St., Pittsburg, Pa.
April,	'92.	Johnson, C. M.,	Care of Park Bros. & Co., Ltd. Pittsburg, Pa.
March,	'92.	Johnston, Robt. G.,	Edith Furnace Co., Allegheny, Pa.
Feb.,	'92.	Johnson, S. H.,	257 Fisk St. Pittsburg, Pa.
April,	'81.	Jones, B. F.,	Jones & Laughlin, Ltd., Pittsburg, Pa.
March,	'88.	Jones, W. Larimer,	Jones & Laughlin, Ltd., Pittsburg, Pa.

DATE OF MEMBERSHIP.			
Feb.,	'92.	Jordan, L. E.,	Bellevue, Pa.
Dec.,	'91.	Kane, Cornelius,	Duquesne Way and Cecil Ave. Pittsburg, Pa.
Nov.,	'80.	Kaufman, Gustave,	813 Hamilton Building, Pittsburg, Pa.
May,	'80.	Kay, J. C.,	Kay Bros. & Co., Water St., Pittsburg, Pa.
Feb.,	'85.	Kay, Jas. I.,	96 Diamond St., Pittsburg, Pa.
Sept.,	'92.	Keeler, Jas. E.,	Observatory, Allegheny, Pa.
March,	'85.	Kelly, J. A.,	28th and Smallman Sts., Pittsburg, Pa.
Jan.,	'85.	Kelly, J. W.,	Box 196, New Brighton, Pa.
Oct.,	'91.	Kelly, M. B.,	61 Wylie Ave., Pittsburg, Pa.
April,	'92.	Kemery, Philo,	Crescent Steel Works, Pittsburg, Pa.
Feb.,	'91.	Kemler, W. H.,	Ashland, Ky.
May,	'86.	Kennedy, Julian,	Room 38, 103 4th Ave., Pittsburg, Pa.
March,	'92.	Kennedy, S. A.,	Isabella Furnace Co., Pittsburg, Pa.
Sept.,	'82.	Kenyon, L. H.,	Pittsburg Locomotive Wks., Allegheny, Pa.
March,	'89.	Kerr, A. C.,	98 3d Ave., Pittsburg, Pa.
June,	'88.	Kimball, Frank I.,	Greensburg, Pa.
Feb.,	'82.	King, T. M.,	B. & O. R. R., Baltimore, Md.
April,	'92.	Kirby, G. G.,	Eliza Furnace Co., Pittsburg, Pa.
March,	'82.	Kirk, Arthur,	Sharpsburg, Pa.
Nov.,	'87.	Kirtland, A. P.,	Cheswick, Allegheny Co., Pa.
April,	'87.	Klages, Geo. W.,	130 S. 11th St., Pittsburg, Pa.
Dec.,	'92.	Klindworth, J. L.,	239 Chartiers St., Allegheny, Pa.
April,	'87.	Koch, Walter E.,	Sharpsburg, Pa.
March,	'92.	Kuntz, J. F.,	Wilkins & Davison, Pittsburg, Pa.

DATE OF MEMBERSHIP.		
Feb., '92.	Lamb, Geo.,	Spang Steel and Iron Wks., Sharpsburg, Pa.
Nov., '88.	Langley, J. W.,	845½ Fairmount St., Cleveland, O.
Oct., '92.	Lash, Horace W.,	Carbon St'l Co., Pittsburg, Pa.
May, '85.	Lauder, Geo.,	48 5th Ave., Pittsburg, Pa.
June, '88.	Lean, D. R.,	Penn Building, Pittsburg, Pa.
Jan., '88.	Leech, Louis D.,	Stevenson Building, Pittsburg, Pa.
April, '84.	Leishman, J. A. G.	42 5th Ave., Pittsburg, Pa.
May, '80.	Leschorn, Alex.,	Phoenix Bridge Co., Phoenixville, Pa.
March, '80.	Lewis, John L.,	Lewis Foundry and Machine Co., Ltd., Pittsburg, Pa.
April, '80.	Lewis, W. J.,	Linden Steel Co., Ltd., Pittsburg, Pa.
May, '90.	Lewis, Harry J.,	Times Building, Pittsburg, Pa.
April, '92.	Lihme, C. B.,	Mansfield Valley, Pa.
Feb., '82.	Lindenthal, Gustav,	45 Cedar St., New York, N.Y.
Feb., '92.	Liggett, Dudley S.,	Sewickley, Pa.
Oct., '88.	Linkenheimer, A.E.	Room 61, Eisner & Phillips Building, Pittsburg, Pa.
Sept., '84.	Lloyd, Henry,	Lewis Block, Pittsburg, Pa.
May, '81.	Lloyd, John W.,	H. Lloyd, Sons & Co., Pittsburg, Pa.
Nov., '90.	Lobingier, J. E.,	254 S. Euclid Ave., Pittsburg, Pa.
April, '92.	Loeffler, Geo. O.,	153 Pearl St., Pittsburg, Pa.
Sept., '90.	Ludwig, Jos.,	525 State St., Schenectady, N. Y.
March, '92.	Lyons, Jas. K.,	Keystone Bridge Works, Pittsburg, Pa.
March, '92.	Luty, B. E. V.,	Observatory Hill, Allegheny, Pa.
Jan., '80.	Macbeth, Geo. A.,	Keystone Flint Glass Co., Pittsburg, Pa.

DATE OF MEMBERSHIP.		
Dec., '92.	MacDonald, W. R.,	6 S. Diamond Street, Allegheny, Pa.
Dec., '91.	Macfarren, S. J.,	413 Lewis Bl'k, Pittsburg, Pa.
Nov., '92.	Mackenzie, Jas.,	Carn'gieSt'lCo., Pittsburg, Pa.
April, '81.	Malone, M. L.,	320 5th Ave., Pittsburg, Pa.
Jan., '80.	Martin, Wm.,	P. O. Box 670, Pittsburg, Pa.
April, '92.	Manby, Chas. E.,	532 Shaw's Ave., McKeesport, Pa.
March, '92.	Manning, W. T.,	B. & O. R. R., Baltimore, Md.
April, '92.	Marsh, Geo. A.,	Mansfield Valley, Pa.
Dec., '83.	Mead, Edward,	P. O. Box 548, Louisville, Ky.
Feb., '91.	Means, E. C.,	Westinghouse Electric Co., Pittsburg, Pa.
June, '89.	Mellor, Walter C.,	77 5th Ave., Pittsburg, Pa.
Dec., '92.	Menough, H. S.,	West Penn Steel Works, Allegheny, Pa.
Sept., '90.	Mercader, Camille,	Edgar Thomson Steel Wks., Braddock, Pa.
March, '88.	Mesta, Geo.,	Lewis Block, Room 512, Pittsburg, Pa.
Jan., '80.	Metcalf, Wm.,	136 1st Ave., Pittsburg, Pa.
Jan., '84.	Meyran, L. A.,	Germania Bank Bldg., Pittsburg, Pa.
Sept., '83.	Miles, Geo. K.,	4th & Chestnut Sts., Phila., Pa.
Feb., '82.	Milholland, J. B.,	5th Ave. and Tunnel St., Pittsburg, Pa.
March, '92.	Miller, Robert,	Dunbar Furnace Co., Dunbar, Pa.
Sept., '92.	Miller, Reuben, Jr.	Crescent Steel Works, Pittsburg, Pa.
Jan., '80.	Miller, Reuben,	136 1st Ave., Pittsburg, Pa.
May, '85.	Miller, Wilson,	18 Lincoln Av. Allegheny, Pa.
Oct., '80.	Milliken, A. C.,	Pottsville Iron and Steel Co., Pottsville, Pa.
April, '92.	Mixer, Chas. T.,	Youngwood, Westmoreland Co., Pa.
March, '92.	Mollenauer, F. H.,	Castle Shannon, Pa.

Date of Membership.		
April, '92.	Mohr, Jacob A.,	Oliver Iron and Steel Co., Pittsburg, Pa.
March, '92.	Moorhead, J. K.,	2d Ave. and Brady St., Pittsburg, Pa.
April, '81.	Moorhead, M. K.,	Moorhead-McClean Co., Pittsburg, Pa.
April, '90.	Morgan, Wm.,	80 Water St., Pittsburg, Pa.
Oct., '86.	Morris, G. W.,	P. O. Box 656, Pittsburg, Pa.
Jan., '90.	Morris, A. Saunders	Westinghouse Electric Co., Pittsburg, Pa.
May, '83.	Morse, Henry C.,	Edgemoor Bridge Works, Wilmington, Del.
March, '90.	Mueller, Gustave,	36 Morgan St., Allegheny, Pa.
April, '80.	Munro, Robert,	23d and Smallman Sts., Pittsburg, Pa.
March, '92.	Murray, J. P.,	Perrysville Ave., Allegheny, Pa.
Sept., '92.	Muse, W. G.,	West Newton, Pa.
April, '92.	Myers, Uriah H.,	Jefferson Iron Wks., Steubenville, O.
March, '80.	McCandless, E. V.,	209 Wood St., Pittsburg, Pa.
Jan., '91.	McClintock, H. P.,	South Ave. and Snowden St., Allegheny, Pa.
Oct., '92.	McClintic, H. H.,	220 Main St., Pittsburg, Pa.
May, '85.	McConnell, Jno. A.	87 Water St., Pittsburg, Pa.
March, '81.	McCulley, R. L.,	161 5th Ave., Pittsburg, Pa.
Feb., '81.	McCune, John D.,	98 4th Ave., Pittsburg, Pa.
May, '89.	McDonald, John,	239 44th St., Pittsburg, Pa.
Dec., '89.	McDonald, C. I.,	143 Fayette St., Allegheny, Pa.
April, '87.	McDowell, James,	Observatory Ave., Allegheny, Pa.
Oct., '90.	McFarland, N. J.,	Pittsburg, Pa.
March, '92.	McGary, Daniel,	Care of Painter & Sons Co., Pittsburg, Pa.

Date of Membership.		
Dec., ’90.	McIntyre, J. B.,	109 E. Pittsburg St., Greensburg, Pa.
Jan., ’91.	McKaig, Thos. B.,	Ferguson Bldg., Pittsburg, Pa.
Dec., ’92.	McKee, C. G.,	806 N. Negley Ave., Pittsburg, Pa.
March, ’92.	McKelvy, J. P.,	Mackintosh, Hemphill & Co., Pittsburg, Pa.
April, ’92.	McKenna, Alex. G.	159 Robinson St., Allegheny, Pa.
Sept., ’80.	McKinney, J. P.,	141 Fayette St., Allegheny, Pa.
Jan., ’83.	McKinney, R. M.,	Elizabeth, Pa.
Feb., ’82.	McMurtry, G. G.,	P. O. Box 960, Pittsburg, Pa.
Feb., ’85.	McQuiston, Jas.,	26th and Railroad Sts., Pittsburg, Pa.
March, ’81.	McRoberts, J. H.,	400 Grant St., Pittsburg, Pa.
Jan., ’80.	Naegeley, John,	Room 9, Renshaw Bldg., Pittsburg, Pa.
Dec., ’92.	Neale, John C.,	Keystone Bridge Works, Pittsburg, Pa.
Jan., ’86.	Nevins, Rich’d, Jr.,	Seattle, Washington.
Nov., ’91.	Nichols, Chas. H.,	Keystone Bridge Works, Pittsburg, Pa.
Jan., ’81.	Nichols, T. B.,	223 Allegheny Ave., Allegheny, Pa.
March, ’92.	Nicholson, J. H.,	88 Penna. Av., Allegheny, Pa.
April, ’80.	Nimick, F. B.,	Singer, Nimick & Co., Ltd., Pittsburg, Pa.
Feb., ’82.	Noble, Patrick,	200 Market Street, San Francisco, Cal.
Nov., ’92.	Nurich, Alex.,	243 Fisk St., Pittsburg, Pa.
Nov., ’92.	Oakley, Frank T.,	Keystone Bridge Works, Pittsburg, Pa.
March, ’92.	Opsion, Ole C.,	Shiffler Bridge Co., Pittsburg, Pa.
Feb., ’83.	Paddock, Jos. H.,	Connellsville, Pa.

DATE OF MEMBERSHIP.		
Nov., '90.	Page, Benj.,	Monon. Con. R. R., 3d Ave. & Try St., Pittsburgh, Pa.
Nov., '92.	Page, Geo. S.,	Park Bros. & Co., Pittsburg, Pa.
May, '89.	Paine, G. H.,	Swissvale, Pa.
March, '84.	Painter, Park,	J. Painter, Sons & Co., Pittsburg, Pa.
Nov., '88.	Palmer, W. P.,	37 Beech St., Allegheny, Pa.
April, '92.	Palmer, Irving A.,	Mansfield Valley, Pa.
Sept., '88.	Park, J. G.,	Room 90, Westinghouse Bldg., Pittsburg, Pa.
Jan., '80.	Parkin, Chas.,	Parnassus, Pa.
April, '84.	Parkin, Walter F.,	136 1st Ave., Pittsburg, Pa.
Dec., '91.	Parsons, B. H.,	Pittsburg Bridge Co., Pittsburg, Pa.
March, '92.	Patterson, P. C.,	McKeesport, Pa.
Feb., '81.	Patterson, Peter,	National Tube Works, McKeesport, Pa.
Nov., '81.	Paul, J. W.,	Verona Tool Wks., 7th Ave. & Liberty St., Pittsburg, Pa.
April, '92.	Pease, F. N.,	P. R. R., Altoona, Pa.
Sept., '83.	Peebles, Andrew,	Schmidt & Friday Bldg., Pittsburg, Pa.
Jan., '80.	Phillips, F. C.,	344 Ridge Av., Allegheny, Pa.
Jan., '83.	Phipps, Henry, Jr.,	Carnegie, Phipps & Co., Ltd., Pittsburg, Pa.
March, '92.	Pierce, Chas. J.,	Western Tube Co., Kewanee, Ill.
Dec., '81.	Porter, John C.,	Spang Steel and Iron Co., Pittsburg, Pa.
May, '87.	Porter, John E.,	Penn Ave. and 2d St., Pittsburg, Pa.
Jan., '83.	Prentice, W. J.,	1009 Liberty Street, Pittsburg, Pa.
Nov., '92.	Probert, R. H.,	269 Main St., Pittsburg, Pa.
Dec., '88.	Purves, Jas.,	Pineville, Ky.

DATE OF MEMBERSHIP.		
Jan., '80.	Quincy, Walter C.,	3d Ave. and Try Street, Pittsburg, Pa.
March, '81.	Ramsey, Jos., Jr.,	Asst. to Pres. C.C.C. I. and St. L. Ry., Cincinnati, O.
Dec., '90.	Randolf, Alfred,	53 Carson St. Pittsburg, Pa.
Jan., '80.	Reed, Jas.,	West Penn. R. R., Allegheny, Pa.
Dec., '89.	Reed, J. R.,	150 Fayette Street, Allegheny, Pa.
Jan., '80.	Rees, Thos. M.,	J. Rees & Sons, Pittsburg, Pa.
June, '88.	Reinman, A. L.,	45 Linden Street, Brooklyn, N. Y.
May, '83.	Reno, Geo. E.,	90 4th Ave., Pittsburg, Pa.
Jan., '80.	Rhodes, Joshua,	Penna. Tube Works, Pittsburg, Pa.
Oct. '91.	Richards, G. T.,	Singer Bld., Pittsburg, Pa.
Jan., '80.	Ricketson, John H.	10 Wood St., Pittsburg, Pa.
Jan., '92.	Riddle, Walter,	287 Ridge Ave., Allegheny, Pa.
April, '87.	Rider, Percy S.,	6 9th St., Pittsburg, Pa.
Jan., '88.	Robbins, F. L.,	Penn Bldg., Pittsburg, Pa.
Jan., '80.	Roberts, Thos. P.,	Craig St., N. of Centre Ave., Pittsburg, Pa.
Jan., '80.	Rodd, Thos.,	Penna. Co., Pittsburg, Pa.
April, '92.	Ross, F. G.,	153 Forbes Avenue, Pittsburg, Pa.
Nov., '92.	Rousseau, H. H.,	31 Sands Street, Brooklyn, N. Y.
Nov., '89.	Ruhe, C. H. W.,	1423 Bluff St., Pittsburg, Pa.
Jan., '88.	Rund, Edwin,	Fuel-Gas and Mfg. Co., Pittsburg, Pa.
May, '91.	Sawyer, Thos. I. J.,	Garrison St., Allegheny, Pa.
April, '84.	Scaife, O. P.,	119 1st Ave., Pittsburg, Pa.
March, '83.	Scaife, W. Lucien,	28th and Smallman Sts., Pittsburg, Pa.
Sept., '87.	Scaife, W. Marcelin,	336 Ridge Av., Allegheny, Pa.

DATE OF MEMBERSHIP.			
Feb.,	'82.	Schellenberg, F.Z.,	Sewickley, Pa.
Jan.,	'80.	Schinneller, Jacob,	Room 31, McClintock Blk., Pittsburg, Pa.
Feb..	'85.	Schmid, Albert,	Westinghouse E. & M. Co., Pittsburg, Pa.
May,	'83.	Schook, Levi,	1st Ave. and Ferry St., Pittsburg, Pa.
March,	'92.	Schluederberg,G.W.	374 Ridge Av., Allegh'ny, Pa.
Sept.,	'92.	Schade, C. G.,	142 43d St., Pittsburg, Pa.
Feb.,	'92.	Scott, Jas.,	Lucy Furnace Co., Pittsburg, Pa.
Sept.,	'82.	Schultz, C. J.,	Iron City Bridge Works, Pittsburg, Pa.
Nov.,	'81.	Schwartz, F. H.,	5000 Liberty Avenue, Pittsburg, Pa.
March,	'84.	Schwartz, J. E.,	61 4th Ave., Pittsburg, Pa.
April,	'90.	Scott, Chas. F.,	Westinghouse E. & M. Co., Pittsburg, Pa.
Feb.,	'91.	Scott, Ernest K.,	Keystone Bridge Works, Pittsburg, Pa.
Sept.,	'90.	Scott, James B.,	122 2d Ave., Pittsburg, Pa.
Jan.,	'83.	Seaver, J. W.,	60 Cedar Ave., Allegheny, Pa.
Jan.,	'89.	Shaw, A. G.,	County Eng. Office, Pittsburg, Pa.
Jan.,	'89.	Shaw, W. W.,	County Engineer's Office, Pittsburg, Pa.
Nov.,	'85.	Shultz, O. G.,	Box 65, McKee's Rocks, Pa.
Feb.,	'92.	Siebenimann, F. E.,	404 Lewis Block, Pittsburg, Pa.
Dec.,	'87.	Simpson, Jas. H.,	249 40th St., Pittsburg, Pa.
Sept.,	'80.	Singer, Harton G.,	83 Water St., Pittsburg, Pa.
May,	'90.	Singer, R. R.,	111 4th Ave., Pittsburg, Pa.
Sept.,	'80.	Singer, W. H.,	Singer, Nimick & Co., Ltd., Pittsburg, Pa.
April,	'92.	Slocum, F. S.,	Care Jones & Laughlins, Ltd., Pittsburg, Pa.

DATE OF MEMBERSHIP.		
Feb., '92.	Smith, Wm.,	Spang, Chalfant & Co., Etna, Pa.
Jan., '90.	Smith, F. S.,	Westinghouse Electric Co., Pittsburg, Pa.
Oct., '92.	Snyder, Chris. H.,	Keystone Bridge Works, Pittsburg, Pa.
Feb., '80.	Snyder, Antes,	Eng. Right of Way, P. R. R., Blairsville, Pa.
April, '84.	Snyder, W. P.,	German Nat. Bk. Bldg., Pittsburg, Pa.
May, '81.	Stafford, C. E.,	Shoenberger & Co., Pittsburg, Pa.
May, '83.	Stevenson, D. A.,	Room 6, Union Station, Pittsburg, Pa.
Jan., '86.	Stevenson, Wm. S.,	Fairmount, W. Va.
April, '92.	Stahl, Karl F.,	57th St. and A. V. R. R., Pittsburg, Pa.
Nov., '82.	Stewart, Geo. R.,	100 Wood St., Pittsburg, Pa.
Oct., '86.	Stewart, J. H.,	F. F. Vandervort & Co., Lewis Blk., Pittsburg, Pa.
Jan., '80.	Stillburg, J. H.,	20 5th Ave., Pittsburg, Pa.
Jan., '90.	Stillwell, L. B.,	Westinghouse Electric Co., Pittsburg, Pa.
Oct., '90.	Stowe, H. C.,	Room 802, 1 Union Square, New York, N. Y.
Jan., '80.	Strobel, C. L.,	Home Insurance Bldg., Chicago, Ill.
Feb., '91.	Stupakoff, S. H.,	U. S. & S. Co., Swissvale, Pa.
Nov., '91.	Sugden, C. H.,	221 40th St., Pittsburg, Pa.
Feb., '83.	Swan, Robert,	39 North Av., Allegheny, Pa.
April, '87.	Swensson, Emil,	Keystone Bridge Works, Pittsburg, Pa.
March, '92.	Sivertsen, O.,	612 Lewis Block, Pittsburg, Pa.
Dec., '92.	Sutton, Stansbury,	284 Ridge Avenue, Allegheny, Pa.

Date of Membership.		
Oct., '91.	Tallman, F. G.,	108 4th Ave., Pittsburg, Pa.
April, '80.	Taylor, E. B.,	1003 Penn Av., Pittsburg, Pa.
Feb., '84.	Taylor, B. H.,	Edgar Thomson Steel Works, Braddock, Pa.
March, '92.	Taylor, Selwyn M.,	420 Bissel Block, Pittsburg, Pa.
Dec., '90.	Temple, W. C.,	408 Lewis Block, Pittsburg, Pa.
May, '86.	Tener, Geo. E,	Edith Furnace Co., Allegheny, Pa.
April, '92.	Thomas, Wm. A.,	Thomas Furnace Co., Niles, Ohio.
April, '89.	Thorsell, J. A.,	119 1st Ave., Pittsburg, Pa.
April, '91.	Tebbetts, C. H.,	68 6th Ave., Pittsburg, Pa.
March, '92.	Townsend, Chas. W.	81 Westinghouse Building, Pittsburg, Pa.
March, '91.	Tone, S. LaRue,	108 4th Ave., Pittsburg, Pa.
Dec., '90.	Tonnelé, Theo.,	McKeesport, Pa.
Jan., '80.	Trimble, Robert,	Penna. Co., Pittsburg, Pa.
Dec., '92.	Thomson, F. DuP.,	Box 524, Wilkinsburg, Pa.
Feb., '81.	Utley, Edwd. H.,	48 5th Ave., Pittsburg, Pa.
Feb., '92.	Vandivort, Theo.,	Blairsville, Pa.
May, '85.	Verner, M. S.,	Supt. Citizens' Traction Co., 939 Penn Av., Pittsburg, Pa.
Dec., '92.	Vierheller, Ph.,	Keystone Bridge Works, Pittsburg, Pa.
April, '92.	Voorhees, Saml. S.,	Altoona, Pa.
Dec., '92.	VanMarken, J. C.,	116 Water St., Pittsburg, Pa.
April, '82.	Wainwright, J.,	111 4th Ave., Pittsburg, Pa.
Jan., '80.	Walker, J. W.,	240 44th St., Pittsburg, Pa.
Dec., '91.	Walker, Robt. L.,	Room 317, Lewis Blk., Pittsburg, Pa.
May, '92.	Walker, Hepburn,	22d and Railroad Sts., Pittsburg, Pa.
Jan., '83.	Warden, C. F.,	Greensburg, Pa.

Date of Membership.		
Nov., ’91.	Warrington, H. E.	C. N. & T. P. Ry., Cincinnati, O.
Jan., ’80.	Weeks, Jos. D.,	Box 1547, Pittsburg, Pa.
April, ’87.	Weiskopf, S. C.,	46 Wall St., New York.
April, ’92.	Wendt, E. F.,	P. & L. E. R. R., New Brighton, Pa.
Feb., ’82.	Westerman, Thos.,	Box 1, Hulton, Pa.
May, ’83.	White, T. S.,	Beaver Falls, Pa.
May, ’92.	White, F. L.,	151 43d St., Pittsburg, Pa.
May, ’80.	Wickersham, S. M.,	Home St., Allegheny, Pa.
Oct., ’80.	Wickersham, Thos.,	Park Bros. & Co., Pittsburg, Pa.
Oct., ’90.	Wieland, C. F.,	Bignall & Keeler Mfg. Co., St. Louis, Mo.
May, ’86.	Wierman, Victor,	Eng. Pgh. Div. P. R. R., Pittsburg, Pa.
Feb., ’80.	Wightman, D. A.,	154 Locust Street, Allegheny, Pa.
Jan., ’80.	Wilcox, John F.,	Lewis Block, Pittsburg, Pa.
May, ’87.	Wilkins, W. G.,	Westinghouse Building, Pittsburg, Pa.
March, ’92.	Weed, Henry T.,	82 Robinson Street, Allegheny, Pa.
Dec., ’92.	White, Wm., Jr.,	Times Bldg., Pittsburg, Pa.
March, ’92.	Whitman, Ph. R.,	215 Emerson Street., Pittsburg, Pa.
April, ’92.	Williams, John E.,	Mansfield Valley, Pa.
April, ’92.	Williams, Fred. H.,	Riverside Iron Works, Wheeling, W. Va.
Jan., ’86.	Wilson, H. M.,	314 Duquesne Way, Pittsburg, Pa.
Jan., ’88.	Wilson, F. T.,	Hanover, Ohio.
Jan., ’88.	Wilson, W. R.,	319 Lewis Block, Pittsburg, Pa.
April, ’92.	Wilson, Jos. M.,	N. 3d St., Steubenville, O.
Feb., ’88.	Winn, Isaac,	National Rolling Mill, McKeesport, Pa.

Date of Membership.		
Jan., '80.	Witherow, J. P.,	Lewis Block, Pittsburg, Pa.
Sept., '80.	Wood, Richard G.,	McKeesport, Pa.
Jan., '88.	Wood, Jos.,	Genl. Manager Pa. Lines, Pittsburg, Pa.
April, '92.	Wood, E. F.,	Munhall, Pa.
Jan., '88.	Woods, Leonard G.,	East End Hotel, Pittsburg, Pa.
Jan., '80.	Zimmerman, W. F.	U. S. Electric Company, Newark, N. J.

EXCHANGES.

Academy of Natural Sciences, Philadelphia, Pa.
American Chemical Journal, 97 Water St., New York.
American Institute Electrical Engineers, 5 Beekman St., New York.
American Institute of Mining Engineers, Lock Box 223, New York.
American Journal of Railway Appliances, 113 Liberty St., New York.
American Scientific Society, 219 River St., Troy, N. Y.
American Society of Civil Engineers, 127 East 23d St., New York.
American Society of Mechanical Engineers, 60 Madison Ave., New York.
Annales des Mines, Paris, France.
Associadad dos Engenheiros Civis Portuguezos, Lisboa, Portuguezos.
Boston Society of Engineers, City Hall, Boston, Mass.
California State Mining Bureau.
Civil Engineers' Club of Cleveland, Cleveland, O.
Club de Engenharia, Rio de Janeiro, Brazil, S. A.
Colorado Scientific Society, Denver, Colorado.
Conn. Assoc. of Civil Engineers & Surveyors, Birmingham, Conn.
Cunningham, E. S. Columbia, Boone Co., Mo.
Drown, T. M., M.D., Mass. Inst. of Technology, Boston, Mass.
Engineering and Mining Journal, 27 Park Place, New York.
Engineering Magazine, World Building, New York.
Engineers' Club, Kansas City, Mo.
Engineers' Club of St. Louis, St. Louis, Mo.
Engineers' Club of Philadelphia, 1122 Girard St., Philadelphia, Pa.

Engineers' Club, per Col. John McLeod, Louisville, Ky.
Engineering Department, Vanderbilt University,
Nashville, Tenn.
E. Ingeniero Civil,
424 Corrientes, Buenos Aires, Argentine Republic, S. A.
Engineering Mechanics, 430 Walnut St., Philadelphia, Pa.
Engineering News, Tribune Building, New York.
Engineering Record, 277 Pearl St., New York.
Fairchild, D. G., Geneva, N. Y.
Franklin Institute, 18 S. Seventh St., Philadelphia, Pa.
Gordon, Henry A., Inspecting Engineer,
Wellington, New Zealand.
Howe, M. H., Prof., Dept. of C. E., Rose Polytech. Institute,
Terre Haute, Indiana.
Indiana Society of Civil Engineers and Surveyors,
Remington, Ind.
Institution of Civil Engineering,
25 Great George St., Westminster, London, S. W.
Iowa Society of Engineers and Surveyors, Glenwood, Iowa.
Iron Age, 98 Reade St., New York.
Iron and Steel Institute, Victoria Mansions,
Victoria St., London, E. C.
Johnson, J. B., Index Dept. Jour. Ass. Eng. Society,
Washington University, St. Louis, Mo.
Journal of Association of Engineering Societies,
73 Broadway, New York.
Journal of Society of Arts, John St., Adelphi, London, S. W.
Library of Second Geological Survey of Pennsylvania,
P. O. Bldg., 4th floor, Room 18, Philadelphia, Pa.
Liverpool Engineering Society,
Colquit St., Liverpool, England.
London Patent Office, London, England.
Manufacturers' Record, Baltimore, Md.
Massachusetts Institute of Technology,
Department of Civil Engineering, Boston, Mass.
Mendenhall, T. C., U. S. Coast Survey Office, Washington, D. C.

Merriman, Mansfield, Prof., Dept. C. E.,	Lehigh University, S. Bethlehem, Pa.
Mineral Products of the United States,	Department of the Interior, Washington, D. C.
Ohio Society of Surveyors and Civil Engineers,	Massillon, O.
Railroad and Engineering Journal,	46 Broadway, New York.
Railway Review,	Chicago, Ill.
Science Club of Portland,	Portland, Oregon.
Sibley College,	Cornell University, Ithaca, N. Y.
Smithsonian Institute,	Washington, D. C.
Sociedad Cientifica Argentina,	Buenos Aires, S. A.
State Association of Engineers,	Norwich, Conn.
Stevens Institute of Technology,	Hoboken, N. J.
Svenska Teknologföreningen,	Stockholm, Sweden.
Swedish Society of Civil Engineers,	Stockholm, Sweden.
Thayer School of C. E.,	Hanover, New Hampshire.
The Technic,	Ann Arbor, Mich.
The Technical Society of the Pacific Coast,	Room 56, Academy of Sciences, San Francisco, Cal.
The Practical Mechanic,	Worcester, Mass.
Thompson, B.,	Box 430, Chattanooga, Tenn.
University of Illinois,	Champaign, Illinois.
University of Pennsylvania,	Philadelphia, Pa.
United States Geological Survey,	Washington, D. C.
Western Society of Engineers,	78 LaSalle St., Chicago, Ill.
Western Railway Club,	The Rookery, Chicago, Ill.
Whitman, J. M.,	Industrial University, Fayetteville, Ark.
Western University of Pennsylvania,	Allegheny, Pa.

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